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Art. XXVII.—*On the Eagre of the Tsien-Tang;* by D. J. Macgowan, M.D.*

No one who has witnessed under favorable circumstances, an Eagre,† or the tidal phenomenon called from its Indian designation a "Bore," can be surprised at the account given by Arrian of the tide near the mouth of the Indus, or at the astonishment manifested by Alexander and his army when they first beheld its action. On the Hooghly branch of the Ganges, where I first saw the Bore, it presented no feature calculated to excite wonder; but on the Tsien-tang, it struck me as being one of the grandest spectacles in nature.

A Chinese Proverb reckons three wonders of the world,—the tides at Hang-chau, the thunder in Lung-chau,‡ and the demons

* From the Transactions of the China Branch of the Royal Asiatic Society.—Read to the Society, 12th January, 1853.

† "His manly heart, whose noble pride
Was still above
Dissembled hate or varnish'd love,
Its more than common transport could not hide;
But like an *eagre* rode, in triumph o'er the tide."—*Dryden.*

‡ Lungchau, (thunder district) is a long mountainous peninsula in Canton province opposite the island of Hainan, and is celebrated throughout China for several myths respecting its thunder storms, which doubtless reverberate through the alpine regions of that latitude in a manner to awaken awe and superstition. Standard Encyclopædias, quoting from various authors on the subject, inform us, that after thunder storms black stones are found, emitting light and a sonorous sound on being struck. At times, also, hatchet-shaped things are picked up which are useful amulets. The

at Tang-chau.* To the first named marvel, the province of Chih-kiang owes its name: that it has not been mentioned in the Travels of Marco Polo, is to be accounted for from the circumstance, that the phenomenon did not occur during any of his visits to the city of Hang-chau: otherwise it would not have remained until the present day undescribed, nor would a distinguished geographer have stated, that "In the North Pacific we have neither the bores of a Hooghly nor the terrific tides of a bay of Fundy."†

The Tsien-tang river, originally called Chih-kiang, takes its rise in a mountainous region where the provinces of Kiang-si, Fuh-kien, and Chih-kiang are long conterminous, and after pursuing a northeasterly course, disembogues into the Hang-chau Bay, a short distance below the city of that name. It is about 220 miles long, flowing more than two-thirds of its way through a mountainous country; the remainder, draining a low alluvial plain. A considerable quantity of silt is conveyed by it to the sea, thus aiding the Yang-tsz' and Yellow Rivers in extending this part of the continent eastward. Being broad, shallow, and rapid, it is navigated with difficulty by flat bottomed vessels, which seem admirably constructed for that purpose. They are drawn by boatmen against the stream, with ropes several hundred feet in length attached to the topmast, those on board being engaged in poling. At Hang-chau, it is about four miles wide; and at its mouth, about ten miles distant, the width is seven. The configuration of a river, however, has little effect in giving rise to Eagres, the cause of these phenomena must be sought for in the form of its estuary, where is presented in a remarkable degree the peculiar features necessary to a sudden elevation of the tidal wave,—a shallow and gradually contracting bay, subject to high tides, at the embouchure of a river more or less obstructed by sands.

fields are often furrowed by thunder as if they had been ploughed. In a temple consecrated to the Thunder Duke, the people annually place a drum, drawn thither on a carriage purposely constructed, which it is supposed he beats during a storm; and it is said, that since a drum covered with paper has been substituted for one covered with leather, the peals of thunder have been less severe. Formerly the drum was placed on the top of a mountain, and a boy left there as an attendant on the thunderer—a sort of sacrifice to him.

* Tang-chau is not far from the birth-place of Confucius, a peninsula of the Shantung Promontory. Thither it is believed the spirits of the departed repair, until summoned to judgment in Hades. Innumerable accounts are given of the pranks of ghosts, which are for a season at liberty in that region. The "ghost's lantern," *ignis fatuus*, is often seen there.

† Vide *Johnston's Physical Atlas of Natural Phenomena*. This splendid and usually accurate work contains a few slight errors in treating of this *terra incognita*. The lofty mountain chain passing through the entire length of Formosa, is omitted in the Chart of the mountain chains of Asia. A very large space in the Geological Chart should have been colored to represent the extensive district of pure alluvium between 30.15 and 35. N. extending from the sea a great distance comprising an area of about eighty thousand square miles, destitute even of a pebble or grain of sand.

Hang-chau Bay, (a foreign designation, Chinamen not distinguishing it from the sea,) is an indentation in the extreme eastern part of the Chinese coast, where the continent converges towards the Pacific, as if to form a cape; its northern headland is Yang-tsz' Cape, an alluvial flat, formed by the great river of that name; the opposite headland is of a very different character, being formed by the termination, at Ketow Point, of the Nain-ling chain of mountains. Thus, on one side the shore is so low, and the bay so shallow as to render it perilous to navigators, while at the other, there is a bold coast and deep water. From Ketow Point to Yang-tsz' Cape, the distance is sixty miles, which is the width of the estuary. A line intersecting it, drawn from the mouth of the river, would be about seventy-five miles. A portion of the Chusan Archipelago is included in the southeastern part of the bay. Although these islands are generally separated by deep channels, the descent of the submarine ground on this part of the coast is extremely gradual, owing to the ever accumulating detritus of the great rivers. The bay itself, is very shallow, and constantly decreasing in depth, the channel being wholly obliterated towards the Tsien-tang. At its southern boundary it receives the water of the Ta-hiah or Ningpo river, where the tides are in no respect remarkable; and to the north of this, near the embouchure of the Tsien-tang, Chang-ngo is disembogued, in which river the tides have a great and rapid rise and fall.

Navigators are well acquainted with the force of tidal currents among the Chusan islands, and in the other part of the bay; there is a rapid increase of their velocity as you advance up the funnel-shaped frith, where they rush at sixteen miles an hour. In the neighborhood of Chapoo, where they are more moderate, their flow is between eleven and twelve knots, with a fall of twenty-eight feet. Vessels approaching this part of the coast are thus in danger of being lost among sands without sighting the low shores. Captain Collinson, when endeavoring to find a channel to Hang-chau in the H. C. Steamer *Phlegethon*, experienced a tide of "eleven and a half knots, when nineteen miles distant from the Chapoo Hills, and two from the shore. Traversing the river [estuary] which at this point is about fifteen miles wide, there was no continuous channel found, although there were some deep spots. When the *Phlegethon* was exposed to this tide, she had an anchor down, with a whole cable, (having previously lost an anchor and cable in endeavoring to hug up,) was under her full power of steam, with sails set, and was still driving." Whales are not unfrequently drawn in by this vortex, and left floundering on the strand or in shallow water by the receding wave; some of the accidents recorded of the unfortunate *Cetacea* are worth quoting. * * *

At the upper part of the bay, and about the mouth of the river, the Eagre is scarcely observable, but owing to the very gradual descent of the shore, and the rapidity of the great flood and ebb, the tidal phenomena even here present a remarkable appearance. Vessels, which a few moments before were afloat, are suddenly left high and dry on a strand nearly two miles in width, which the returning wave as quickly floods. It is not until the tide rushes beyond the mouth of the river, that it becomes elevated to a lofty wave constituting the Eagre, which attains its greatest magnitude opposite the city of Hang-chau. Generally there is nothing in its aspect, except on the *third* day of the second month, and on the *eighteenth* of the eighth, or at the spring-tide about the period of the vernal and autumnal equinoxes, its great intensity being at the latter season. Sometimes, however, during the prevalence of easterly winds, on the *third* day after the sun and moon are in conjunction, or in opposition, the Eagre courses up the river with hardly less majesty than when paying its ordinary periodical visit. On one of these unusual occasions, when I was travelling in native costume, I had an opportunity of witnessing it, on December 14, 1848, at about 2 P. M.

Between the river and the city walls, which are a mile distant, dense suburbs extend several miles along the banks. As the hour of flood-tide approached, crowds gathered in the streets running at right angles with the Tsien-tang, but at safe distances. My position was a terrace in front of the Tri-wave Temple, which afforded a good view of the entire scene. On a sudden, all traffic in the thronged mart was suspended, porters cleared the front street of every description of merchandise, boatmen ceased lading and unlading their vessels, and put out into the middle of the stream, so that a few moments sufficed to give a deserted appearance to the busiest part of one of the busiest cities of Asia. The centre of the river teemed with craft, from small boats to huge barges, including the gay "flower boats." Loud shouting from the fleet announced the appearance of the flood, which seemed like a glistening white cable, stretched athwart the river at its mouth, as far down as the eye could reach. Its noise, compared by Chinese poets to that of thunder, speedily drowned that of the boatmen; and as it advanced with prodigious velocity,—at the rate, I should judge, of twenty-five miles an hour,—it assumed the appearance of an alabaster wall, or rather of a cataract four or five miles across, and about thirty feet high, moving bodily onward. Soon it reached the advanced guard of the immense assemblage of vessels awaiting its approach. Knowing that the Bore of the Hooghly, which scarce deserved mention in connection with the one before me, invariably overturned boats which were not skillfully managed, I could not but feel apprehensive for the lives of the floating multitude: as the foaming wall of water

dashed impetuously onwards, they were silenced, all being intently occupied in keeping their prows towards the wave which threatened to submerge every thing afloat; but they all vaulted, as it were, to the summit with perfect safety. The spectacle was of greatest interest when the Eagre had passed about one-half way among the craft. On one side they were quietly reposing on the surface of the unruffled stream, while those on the nether portion were pitching and heaving in tumultuous confusion on the flood, others were scaling with the agility of salmon, the formidable cascade. This grand and exciting scene was but of a moment's duration,—it passed up the river in an instant, but from this point with gradually diminishing force, size, and velocity, until it ceased to be perceptible; which Chinese accounts represent to be eighty miles distant from the city! From ebb to flood tide, the change was almost instantaneous; a slight flood continued after the passage of the wave, but it soon began to ebb. Having lost my memoranda, I am obliged to write from recollection: my impression is, that the fall was about twenty feet; the Chinese say that the rise and fall is sometimes forty feet at Hang-chau. The maximum rise and fall at spring tides is probably at the mouth of the river, or upper part of the bay, where the Eagre is hardly discoverable: in the Bay of Fundy, where the tides rush in with amazing velocity, there is at one place a rise of seventy feet; but there the magnificent phenomenon in question does not appear to be known at all. It is not, therefore, where tides attain their greatest rapidity, or maximum rise and fall, that this wave is met with, but where a river and its estuary both present a peculiar configuration.

Dryden's definition of an Eagre, appended in a note to the verse above quoted in the *Threnodia Augustalis*, is, "a tide swelling above another tide," which he says he had himself observed in the river Trent. Such, according to Chinese oral accounts, is the character of the Tsien-tang tides,—a wave of considerable height rushes suddenly in from the bay, which is soon followed by one much larger. Other accounts represent three successive waves riding in; hence the name of the temple mentioned, that of the Three Waves. Both here and on the Hooghly, I observed but one wave; my attention, however, was not particularly directed to this feature of the Eagre. The term should perhaps be more comprehensive, and express "the instantaneous rise and advance of a tidal wave;" the Indian barbarism "Bore" should be discarded altogether.

A very short period elapsed between the passage of the Eagre and the resumption of traffic; the vessels were soon attached to the shore again, women and children were occupied in gathering articles which the careless or unskillful had lost in the aquatic meleé. The streets were drenched with spray, and a considerable

volume of water splashed over the banks into the head of the grand canal, a few feet distant.

The earlier records of the Chinese would seem to indicate that the beauties of nature had failed to make deep impressions on their imaginations; but from a remote period, men of letters have portrayed nature with true poetic feeling, and in delineating landscape they have particularly excelled. An object, therefore, of such sublime grandeur as the Eagre, occurring frequently at the Athens of China, could not fail to attract the pencil of the literati; it has therefore often been the theme of poetic scholars. No one, not himself capable of clothing the "perception of similitude in dissimilitude," as Wordsworth expresses it, should attempt a version of their artistic effusions; yet an account of this tidal phenomenon would be extremely defective, unless it were presented, to some extent, from a Chinese point of view.

It has been already mentioned, that the Chinese regard the Eagre as one of the wonders of their world, and that it gave name to the province. As might be expected, therefore, it is blended with their mythology. It is not a little remarkable, however, that it should be popularly ascribed to the spiritual energy of a *shin* (or god), who lived so recently as five hundred and forty years before our era, or about twenty years before the birth of Confucius. At that period the Tsien-tang was the boundary of two belligerent kingdoms, Wú and Yueh. Fú Chai, king of the former, incensed against his minister Wú Tsz'si, for opposing the terms of a treaty, submitted by Chung, ambassador of Kú Tsien, king of Yueh, sent him a sword, with which, understanding his master's will, he committed suicide by cutting his throat, a method still pursued by sovereigns in China towards officers of distinction who have incurred their displeasure. This incident in ancient history is recorded in the spring and autumn annals of Confucius; but in a work, entitled "Spring and Autumn Annals of the States of Wú and Yueh," a historical novel, written several hundred years after, a prevailing myth is superadded to the authentic narrative, which the author himself seems to credit, and which to the present day is received as verity.

Wú Tsz'si's corpse, which was thrown into the Tsien-tang, after being carried to and fro by the tide for some time, tunnelled a passage through the hills on the Yueh side, as far as the tomb of the quondam ambassador Chung, whose cadaver he took with him to the estuary. Since that period, it is stated Wú Tsz'si has been the god of the Eagre, his periodical indignation being exhibited by its violence; hence the sacrifices and prayers officially presented at appointed seasons to propitiate his anger. Monarchs of almost every dynasty have honored him with titles, so that they fill half a page of the work in which they are recorded.

Since the apotheosis of the faithful statesman, their penchant for hero-worship has led the people to multiply the tutelary divinities of the tide to a great extent. Thus it has lords many and gods many, so much so that a Pantheon has been erected at Haining, the Temple of the Tidal gods, in which, besides a central image of Wú Tsz'si, there are fifteen others ranged in rows on either side. Some of the most distinguished of these *shin* have temples dedicated to their special honor, and therefore deserve notice in this sketch.

The Tidal King Temple, is in the country. "Its *shin* (or god) was an officer named Stone," who, in 828 A. D., undertook the restoration of a dyke, which an Eagre of unusual violence had overthrown; and failing in the construction of the foundation, drowned himself from chagrin. "He afterwards became a *shin*," (or god,) and three centuries later, on the occasion of a combat between the people and rebels, who were attempting to capture Hang-chau, his name was seen inscribed on a streamer in the darkened sky, where also unearthly noises were heard. The enemy instantly succumbed. Again, a hundred years later, when an extraordinary Eagre destroyed the dykes, occasioning great loss of life and property, the waters retired through prayers and sacrifices to his manes. In consequence of this, a Budhist priest was appointed to the charge of his temple, that a regular religious service might be kept up to his honor.

The Temple of Unanimity. "Its *shin* is Luh kwei," a military officer who flourished in the twelfth century. He fell on the field of battle, and "became a god." Not long after, when the dykes were overthrown by a violent tidal wave, his *shin* led soldiers from Hades and forced back the tide. Subsequently, when it returned with greater force, extending to the city walls and flooding the country, he re-appeared accompanied by three damsels bearing streamers. By their united labor, the stones, which had been carried away, were brought back for a foundation. To commemorate these services, the title of Great Mound Earl, was conferred upon him by the Emperor, who also ennobled the fair attendants. The temple has, besides several stone images, those of the three fairies and twelve horary *shin* (or gods), for alternate officers in the government of the tide.

One more example will suffice: It is of a temple some distance from the city, erected in honor of a tea merchant, who, above seven hundred years ago, devoted a large part of his property to the construction of a dyke, which was overthrown soon after its completion. In despair he cast himself into the swelling wave, which wafted his corpse from place to place; wherever it rested, sand was thrown up, and thus a foundation was provided for another embankment. Posthumous titles were conferred upon him, and a temple erected in which divine honors are paid him.

Despite the superstitions of the populace, which had the sanction of government, there have been philosophical minds, who, without discarding popular myths, speculate on the natural cause of the phenomenon, and propound theories for its explanation. From a summary of these, contained in the Topography of the Prefecture, the following, having reference also to tides in general, have been selected :—

1st, A consequence of the contracting and expanding of the Spring of Nature.

2nd, An enormous Serpent entering into and emerging from oceanic caverns causes their ebb and flow.

3d, The Budhist theory, which attributes them to the transformations of the Divine Dragon.

4th, A consequence of the Sun rising out of the ocean.

5th, From being influenced by the Milky Way.

6th, From being drawn by the Sun.

7th, From the phases of the Moon. According to Plutarch, Pytheas of Marseilles first ascribed tidal oscillation to this orb.

8th, Pulsation of the Earth's blood—water.

9th, Expansion and contraction of the Atmosphere under the influence of the heavens, which occasion a corresponding rise and fall both of land and water: thus, when the waters flood, the earth ebbs, and *vice versa ;* of which landsmen are no more conscious, than persons in a boat are of the tides. In confirmation and illustration of this theory, the fact is adduced, of the rise and fall of the pilous surface of the dried skins of certain sea animals simultaneously with the tide!

By others, the reciprocal action of the dual powers was referred to, as explaining all tidal phenomena. One, like Pliny, says the sun and moon draw the waters after them.

Some, however, more shrewd than the rest, sought for the cause of the peculiar tides of the Tsien-tang, in the configuration of its embouchure, an idea which is combatted by another writer as untenable, because the Ningpo and Chang-ngo river present the same features (which is a mistake), and have no Eagre. It is owing, says this author, to the greater quantity of water which comes into that river. The learned adhere to one or the other of these theories, the populace to that of the serpent, all classes believing that the gods already named are actively concerned in the matter. It should be remembered that until Newton cleared up the subject, very crude notions prevailed respecting it in Europe.

Although the inhabitants of this region are the most timid in the empire, there were among them, at one period, many who from mere bravado would plunge into the frightful wave. A proclamation against the custom still exists, from which it appears that many lives have been lost in attempting this exploit. After appealing to that regard which all should entertain for the body

inherited from their parents, the document exhorts all good people not to go voluntarily to the dread abode of the Sea Dragon; and admonishes them that their souls, should they perish in this manner, will sink into eternal perdition under the Nine Springs of Hades. "Life has its natural limits, why not await the last day fixed by the decree of Heaven? Parents, wife, and children, will be seen gazing from the shore, bewailing your fate, and no condolence can be offered them." Condign punishment is threatened against all who should disregard the admonitions.

The geological changes resulting from these great tides are less than might be expected, owing to the degradation of the land by tidal and fluviatile action elsewhere. Occasionally the land suffers the loss of considerable portions of soil, but it is speedily replaced by the same power, and is ever extending, by accumulations of detritus. There have however been some marked physical changes effected within the historic period, by the combined action of the Eagre, and of the two great rivers of China.

Until about the eleventh century, there was a ledge of rocks projecting above the surface of the stream, near Hang-chau city, known as *Losah* rock, a Sanscrit term employed by Budhists to express *devilish*. Hence that part of the Tsien-tang, taking its name from this obstruction to navigation, was once called the Devilish River. It was large enough to be a place of resort on certain festivals—religious and Thespian. A poet of that period playfully inquires how it happened that the great Yú, whilst opening the river, omitted to perforate a rock a *le* in length. Finally it yielded to the attack of the fierce Eagre, and gradually disappeared.

A smaller rock measuring only 16 feet high, 14 long, and 6 wide, disappeared 145 B. C. Two years after, it was brought into sight again by the same force which had removed it, when it disappeared altogether.

Kam-pu, once a mart of considerable importance, the port of what, at one period at least, was the metropolis and capital of the empire, mentioned by Marco Polo as an extremely fine port, twenty-five miles from the city, frequented by all the ships, that bring merchandise from India, is now an insignificant walled village, in consequence of the receding of the sea, and the filling up of the bay. More than fifty miles distant, another city, Cha-pu, has sprung up near the sea, which, for a long time to come will be the port of Hang-chau; but the Yellow River and the Yang-tsz' are surely, and not slowly, depositing silt, which will render it in its turn inaccessible.

Chinese ingenuity has long been exerted, but with indifferent success, in preserving their alluvial plain from the wasting action of the Eagre. The earliest mention of dykes is about the commencement of our era, when an engineer named Tsau, under-

took the work. Offering a thousand copper cash for every bushel of earth brought for the embankment, he induced laborers to collect in immense numbers, who on being required to bring stones instead of earth, soon disappeared. From this circumstance the work was called Tsien-tang, the *cash* or *money* dyke—a name subsequently applied to the district, and finally to the river, its present designation.

Embankments of earth were generally resorted to, until the middle of the seventh century, when stone was employed; but the frequent inundations which occurred from the tide, shew that the art of constructing dykes was imperfectly understood. The general statistics of the empire ascribe the first erection of dykes to the Prince Wú Shuh, 930 A. D., whose court was at Hang-chau. This Viceroy, according to the records of the Sung dynasty, also employed intimidation and cajolery in defending the land from the violence of the Eagre. First he summoned five hundred daring archers, each of whom was provided with six arrows, which they let fly at the aqueous monster as it came swelling onwards; next he made prayers and offerings in the temple of Wú Tsz'-si; and finally, he composed a hymn, which he enclosed in an envelop, together with the key of the dyke water-gate, and addressing it to the gods generally, threw it into the river; forthwith the waters retired. Frequent reference is made to these proceedings in topographical and other works. Perhaps they were performed merely for the amusement of the people, or, if he had any faith in them, he was too sagacious and practical to confide in anything less than dykes, superior and altogether different from those heretofore employed. Bamboo frames were made and sunk on the banks behind a row of piles, and filled with large stones; and on the foundation thus laid, the embankment was built. To the present day no method has been found superior to that of this prince. The contrivance is not unlike that which western engineers have found most effectual in protecting river banks.

This work endured only sixty years; a tide carried much of it away, laying the country under water, and occasioning devastation. An imperially appointed officer, aided by the military, was occupied seven years in vain attempts to construct a permanent embankment. At length he was dismissed, and his successor, by employing the method mentioned above, completed the work in one year. It however was not owing to superior skill or energy that he accomplished it, but to one of those sudden shifts of the loose bed of the estuary, which have given rise to so many superstitions. According to the statement he made to the emperor, who had expressed a desire to know how the work had been so quickly finished, the engineer, after praying to Wú Tsz'-si, had the satisfaction of procuring his aid; for the next day the waters retired, having raised several miles of yellow sand, which ena-

bled him to accomplish the undertaking. Nevertheless disasters, from the washing away of embankments when a violent storm occurred simultaneously with the Eagre, are recorded during every reign. It was found necessary to have soldiers stationed on its entire length to mark the first point of loosening.

In 1026 A. D., an officer named Tsiang constructed a stone dyke several miles in length, of such excellence and strength, that about seventeen years after the grateful people of Hang-chau erected a temple to him on the embankment.

The emperor Kau-tsung, in 1131, ordered ten iron plates, each weighing above one hundred and thirty pounds, to be employed as charms against the mischievous spirits, in hopes that they would thereby be induced to respect the dykes, which were kept up at so much cost. They were sunk at convenient distances, in front of the embankments; but on the following year, iron charms and stone walls were all carried off, and the loss of life and property was unusually great in consequence. On the following year, the experiment was again tried, the monarch himself writing the charms, according to the forms of the Tauist sect. These iron plates were deposited in chests of choice wood workmanship, and sunk by Tauist priests with great ceremony. The result is not mentioned, but the record goes on detailing numerous repairs following successive disasters.

During the period of the Mongolian reign in China, the destructive action of the tides was remarkable. Kublai, the first emperor of that, the Yuen dynasty, caused repairs to be made; but they could not have been effective, as soon after it is recorded that more than twenty thousand men endeavored in vain to restore the breaches of a few miles. Disasters occurred every year, and repairs were going on incessantly; at length a Mongolian officer (judging from his name) informed the emperor Tai-ting of the experiment made with Tauist charms; and as his majesty approved of the propsition for repeating them, they were doubtless tried.

Just as the Yuen dynasty was drawing to its close, the reigning emperor called upon the head of the Tauist sect to perform his arts to stay the flood; and on the failure of these, he had two hundred and sixty Budhist priests engaged in trying the virtue of their ceremonies for the same purpose, and with the same result. At length after a general consultation among imperial officers, (men who had displayed ability in constructing the Grand Canal,) the whole population was either called out to labor, or required to subscribe funds. Budhists and Tauist priests, it is stated, were not exempted. Embankments, were accordingly constructed, superior to any which had preceded them; and to commemorate the event of their completion, the name of the city In-kwan, at the upper part of the estuary, was changed to Ning-hai, or the "sea tranquillized."

No extensive repairs were called for until twenty years after, but from that time, the fall and rebuilding of dykes are frequently mentioned. After one of these disasters, an ox was sacrificed to the *shin* of the eastern sea, the repairs at this time required thirteen years' labor, and nearly the whole province was called on to aid in defraying the expense.

A typhoon, in 1578, which lasted three days and nights, caused with the tide extensive losses. Roaring or howling noises are mentioned several times in connection with sea storms; they were peculiarly alarming in that typhoon. Salt water covered the fields for many miles, drowning many of the inhabitants. In reconstructing the dykes, unusual care was taken to have stones hewn in such a manner as to afford mutual attachment and support: five were laid lengthwise, and on these, five more were placed crosswise; the wall was then carried up, the stones, which were five feet two inches long, and one foot eight inches in thickness each way, being placed alternately sideway and endwise towards the stream. Some details are given of the cost of these stones. The quarrying and dressing was three mace of silver, one mace for boat-hire and seven candareens for portage,—that is, about seventy-five cents for each stone. The dykes which I saw on the Tsien-tang were perpendicular walls; the Chinese, however, are perfectly aware that a slope towards the water gives additional strength to an embankment; a sea wall at one part of the bay near Chin-hai, is placed a portion of its way at an angle of thirty degrees.

In the reign of Kang-hi in 1665, after a typhoon, like the former of three day's duration, and occurring with an Eagre, the dykes were generally overturned, with much destruction of life and property. In rebuilding them, iron clasps were employed to keep the stones together. This illustrious monarch personally inspected the works, and gave many directions respecting them. During this reign one of those phenomena occurred, which has at different periods in their history struck the people with amazement. Officers and people were bewailing an inroad of the tide which had carried off several miles of country, and were sacrificing to the gods for succor, when another tide suddenly brought the whole back again!

Kang-hi's successor Yung-ching (1723–36,) endeavored by the use of huge amulets and numerous religious ceremonies to appease the gods, although he did not neglect the embankments. Finding that five iron oxen which had been placed at different points of the river and estuary were not likely to prove efficacious talismans, he ordered the erection, at his own expense, of a great temple dedicated to the *Hai-shin* or sea gods, in the city of Haining. A stone slab or monument contains the imperial rescript in relation to the structure. "I find," says his majesty, "that

the ancient sages sacrificed to the gods of the hills and springs, on account of their power of delivering and succoring men; it is proper therefore to seek their favor. They should be revered and recompensed; men's hearts should be excited to regard them with reverential awe, lest they should gradually presume to slight them. In the second year of this reign, when the dykes were overthrown, I sent officers with funds to examine and restore them. I apprehend that the people in general are wanting in reverence for the *shin ming* [gods], and that they are often wanting in piety; I therefore urge you to decorous and respectful demeaor in everything that regards them, and hereby enjoin on the officers to impart instruction to each family, in order that all may be aroused and quickened. Of late years the gods have silently vouchsafed protection, enabling the people to repose in safety, and though this year the tides have been very great, threatening general ruin, yet they happily abated without damage to the embankments, entirely in consequence of the mysterious protection which the gods, having regard for us all, have vouchsafed. I therefore make a special gift from the privy purse of one hundred thousand taels of silver (about $130,000) for the erection of a sea god Temple, that they may therein be honored and recompensed. Let the civil and military officers measure off the ground, and engage men to rear a spacious and magnificent edifice, in which to honor, and sacrifice to, the gods, in acknowledgment for their protecting the people and warding off calamities. Its appearance, moreover, will serve to affect to gratitude those from afar, as well as those who are near, causing them to discard impiety and to become reverential."

In the same spirit the succeeding emperor Kien-lung addressed the people in 1758. Referring to a visit he had made to Hang-chau, when he witnessed the Eagre, he says "surely the protection which the *shin ming* have vouchsafed the city, (since the repairs of the previous reign,) requires the erection of a temple to the gods of the sea, in the capital of the province," and then directs that their power and virtue should be honored accordingly. On another occasion he devoutly thanks the *shin*,* for the favor experienced, and states that he keeps it constantly in mind.

From the foregoing extracts from native authorities it will be seen, that the people of this part of China are in an analogous position to the Hollanders. The subject of dykes has long occupied their attention, and tasked their energies,—on the Hwàng, to resist fluviatile, and on the Tsien-tang, tidal action: volumes have been written on the subject, and a corps of officials are employed specially for observing the wear of embankments.

* *Shin*, is the term adopted by students of Chinese to designate God in this language, conceiving it to be the generic name of the deities of China. Accordingly throughout this paper, *Shin* has been thus translated. It should be added, however, that a majority of the older scholars concur in rendering it by *spirit*, employing the title of *Shang-te* for the chief object of worship,—the *Theos*.

To give an idea of the expense entailed by the tides of Hang-chau Bay, I have cast up the sums disbursed at different times during the reign of Kien-lung, comprising a period of sixty years (1736–96) as detailed in the Topography of Hang-chau.

For the embankment in Hang-chau district:—

	Taels of Silver.
Contributed by officers, ...	4,338,064
Appropriations from Imperial Treasury,	942,800
Contributed by the people, ...	790,526
	6,713,390

or about $130,000 per annum, but as silver is now 20 per cent dearer than it was in that reign, a corresponding reduction should perhaps be made from this sum. There is a coast line of about 180 miles, extending from Chin-hai to Hang-chau which is not included in the above; also the shore of the estuary west of Chapoo, has more or less embankment, to the boundaries of Hang-chau, which requires large outlays. As the greatest force of the tides is experienced on the left, or Hang-chau side of the estuary, it is probable that an expenditure of one-third of the above sufficed to preserve the country from inundation. As the Yellow River is "China's sorrow," so the "difficulty" of Chih-kiang is the Eagre of the Tsien-tang.

ART. XXVIII.—*Observations on Binocular Vision;* by Professor WILLIAM B. ROGERS.

(Continued from p. 220.)

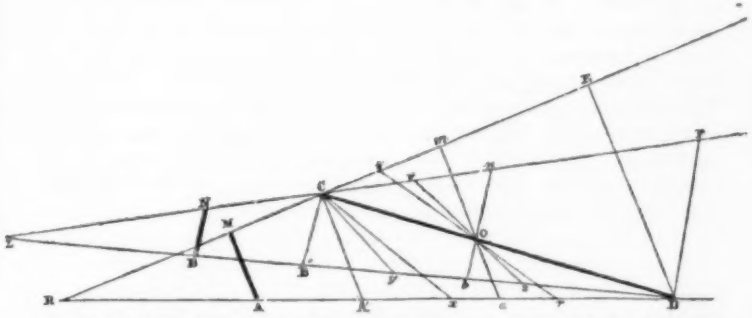
15. *On the apparent relief of the component lines as bearing on the theory of binocular perspective.*

In describing the different phases presented in the combination of inclined lines, special mention was made of the perspectiveness of the intersecting lines of which the resultant is usually formed, but certain features of the phenomenon were purposely omitted as being unessential to the general account. The following observations are intended to trace more precisely the development of the effect and in connexion with other analogous phenomena to furnish suggestions as to the origin in such cases of our perception of relief.

We have seen that when we confine the eyes to any particular stage of convergency, the intersecting lines of the resultant present themselves in a plane parallel to the paper, but when we allow them within certain limits to vary their convergency, these lines assume more or less relief. We have now to add the important fact that *while the lines thus acquire perspectiveness they become longer and more acutely inclined to one another.*

These changes usually go on until the lines assume the greatest amount of relief and the least angle of intersection of which from their positions in the diagram they are susceptible, and then the opposite changes supervene. If but slightly inclined in the diagram, they acquire the perspective position almost as soon as they are brought to intersect. Their inclination to the plane of the paper then rapidly augments, they grow longer and the included angle lessens, until in many cases it becomes imperceptible, and for a moment the two lines seem to melt into a single resultant. But as we look at this we see it again resolving itself into two intersecting lines varying continually in apparent length, mutual inclination and amount of relief. The relation of these various appearances to one another and to the visual conditions in which they occur will be more clearly understood by referring to fig. 27.

27.



In this diagram R and L denote the centres of the right and left eyes AM, and BN, the two lines which are to be combined, and RCED LCFD the planes in which the axes of the right and left eyes respectively revolve while directed to successive points of AM, BN. Since CD is the intersection of the two optical planes, it is obvious that the axes directed to corresponding points of AM BN will always meet in that line. Hence A and B will be seen united at D, M and N at C, and so other points of AM, BN will be combined at some determinate place in CD. This line is therefore the locus of all the points due to the binocular combination of the corresponding points of the component lines AM BN.

If we begin our observations by converging the axes through M and N to C and hold them in this inclination, AM and BN will appear in the position and of the magnitude represented by A′C B′C. If then reducing the convergence of the axes we direct them through A and B to D, the components will appear in DE DF. The resultant figures A′C B′ and DEF being in planes parallel to that of the diagram AM BN, their lines will

have the same mutual inclination in each case as that of AM and BN, although of greater length in proportion to their greater distance from R and L. Lastly, suppose the axes to be steadily converged through some other corresponding points of the component lines so as for example to meet at O, we shall then have in place of AM BN a resultant consisting of the two intersecting lines *am bn* parallel to the former and lying in a parallel plane. Thus to whatever point of CD we may direct the eyes with a steady convergence, the binocular resultant will be seen to consist of two lines intersecting in that point, parallel respectively to the lines of the diagram and of length greater in proportion to their distance.

When however the eyes are allowed to vary their directions so as to combine a number of contiguous points of the component lines in rather quick succession, the resultant intersecting lines will be seen to assume a perspective attitude, so that, taking O for example as the point at which we note their momentary intersection, they will appear in some such positions as *rt sv*, presenting a perspective resultant of which the intersecting lines are longer than *am bn* and inclined to one another at a less angle. When the combination of contiguous points is spontaneously effected with great celerity the lines *rt sv* are seen to approach the position CD and appear even momentarily to coalesce with one another. But as soon as the axial movement is retarded, they pass by rapid gradations to an attitude similar to that of *rt sv* or to that of even less relief, or more nearly approaching that of *am bn*. The same effect is often apparent when the lines of the resultant are seen merely united at the ends without crossing one another, as where the point of intersection of these lines, or which is the same thing, the point of axial convergence, after moving along the contiguous part of CD reaches C or D. In this case we may observe the resultant lines in the position *Cx Cy* or some corresponding attitudes in reference to D.

It can hardly be doubted that the variable and seemingly capricious phenomena just described are dependant on the successive convergence of the axes to different distances, and on the perception resulting from this of a greater or less perspectiveness in the direction of the series of resultant points. It may be assumed that a certain degree of rapidity in effecting this succession of combinations, in other words, this successive intersection of the two lines at differents points of their length and at different distances from the eyes, is necessary to create a distinct and strong perception of relief. For as we have seen, every pause in this movement of the axes is accompanied by a reduction of the relief, and if continued destroys it altogether. Now as the progressive change of convergence of the optic axes in these experiments has the effect of carrying the intersection of

the resultant lines (O for example) along these two lines in the same direction at the same time and with equal velocity, it would seem to be a natural consequence that the feeling of perspectiveness should connect itself equally with both these lines when observed in any momentary phases of intersection. According to this view the continual changes of apparent relief and the corresponding approach or separation of the intersecting lines, would arise from differences in the force and precision of the perspective idea, depending on the greater or less rapidity of the converging movement of the eyes.

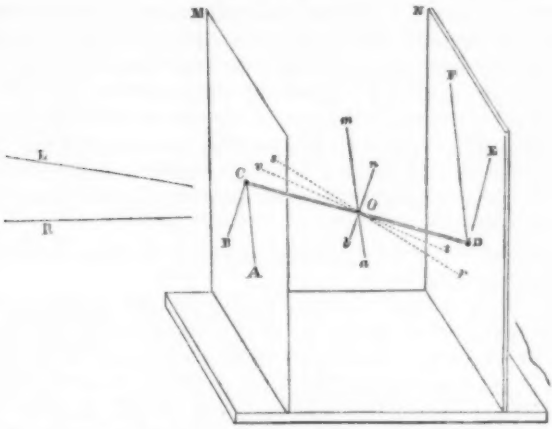
16. *Conditions of the binocular vision of a physical line in a perspective position.*

In describing under a former head (13) the appearances of a physical line or wire placed so as to coincide with the resultant of two inclined lines, particular mention was made of the perspectiveness of the two intersecting images of the wire as well as of the component lines of the resultant. A simple means of studying the former effect is obtained by fastening the point of a penknife into the end of a slender rod of wood (one of Faber's pencils) at right angles to its length, and holding the rod in a steeply sloping position between the eyes and a sheet of paper placed on the table. When the rod has been adjusted so as to be distinctly seen by each eye, as for example in the position CD (fig. 27), the eyes being at R and L, if we converge the axes *steadily* upon C or D or any intermediate point as O, we see the rod double, the two images appearing in a plane passing through C D or O at right angles to the direction of vision. Taking C for the point of axial convergence, the images will have the positions A′C and B′C; for D they will be represented by DE, DF, and for *o* by *am*, *bn*. But if we glance along the rod without making a continued pause at any point of its length, the two images will be seen to have a perspective position, continually varying in amount, at the same time that their mutual inclination and the point of their intersection changes. So long as the rod is held in this steep position in regard to the plane of the paper, the two images never approach so closely as to appear even for a moment to coalesce, and when we incline the rod at a much less angle, although this transient appearance of union often occurs, the general effect is that of two nearly coincident intersecting perspective lines.

Instrument for the binocular analysis of a perspective line.

The following simple apparatus (fig. 28), suggested by the above experiment shows in a very striking manner the variable double appearance of a physical line viewed in the conditions above described. It consists of two rectangular plates M and N, the former of clear glass, the latter of thin board covered with white paper and each about five inches wide by ten high. These

28.



are firmly fastened in a vertical position and parallel to one another, at an interval of 7 inches in a rectangular base of wood. The object used as a physical line is a thin uniform black cord one end of which is temporarily fastened to the inner side of the glass plate at some point C by a small shot-like lump of wax, while the other carried through a hole D in the vertical board is fastened by a similar piece applied behind D on the outer side of the board. Different inclinations are given to the cord by raising or lowering C, fixing it in the new position by the wax and then tightening the cord through the hole D.

It is needless to detail the various appearances of CD to the eyes supposed to be placed at RL, since they are the same as have just been described, with the advantage of being more clearly exhibited. I will merely say in explanation of the figure, that ACB is the aspect presented by the line when the eyes are steadily converged to C, EDF that when they are fixed on D, and *am bn* that when they are fixed on O; and that in the ordinary glancing vision from point to point of CD, it presents the appearance of the two slightly inclined perspective lines *rs tv*.

It can hardly be doubted that in thus viewing a physical line, the sense of perspectiveness associated with each of the two images has its origin in the successive direction of the optic axes to contiguous points, and as precisely the same appearances accompany the binocular union of two lines to form a perspective resultant, we are thus furnished with a confirmation of the theory before suggested that in this latter case the perspectiveness of the components originates from successive convergence to contiguous points along the imaginary resultant line. Thus while the stereoscope presents us the *binocular synthesis* of a perspective line

and the instrument just described gives us its *analysis*, they both lead to the same conclusion as regards the elementary impressions which are combined in the perception of such a line through binocular vision.

It is important to remark that in both these cases *when the component perspective lines are nearly coincident*, the perception to which they give rise in the ordinary rapid mode of vision is that *of a single resultant or a physical line, in a perspective position.* If we imagine such a line to be observed for the first time, there can be no doubt that it would appear confused and double until by looking at it with one eye only, or by turning it into a position transverse to the direction of view, and by applying the sense of touch from end to end, we rectify the impression made by the two nearly coincident intersecting images first perceived. By repetitions of this process, experience permanently connects the idea of a single perspective line with two nearly but not quite coincident intersecting images, of which the point of intersection continually changes its distance from the eyes. In a word, this *double image, peculiar in its form and variations, becomes the optical sign of a perspective line, and whenever impressed upon the eyes, it must by the law of association excite in us the idea of a single line in a perspective position.*

Thus in the case of binocular vision the perception of a perspective line *as single* is necessarily suggested by the two non-coincident pictures, provided their relation to one another is such as has usually accompanied our vision of such a single line. But when the two intersecting images make a considerable angle they depart from these conditions, and hence although as before explained, each appears in more or less of a perspective position, they do not by their united impression suggest the idea of a single perspective line. Thus when we view the cord CD in fig. 28 nearly endwise, we see two greatly divergent images producing a confused impression which, unless helped by information drawn from other sources, would leave us in doubt as to the true form of the object before us. The same uncertainty attends our judgment in regard to the perspective resultant in the stereoscope when its two lines intersect at a considerable angle.

17. *Of the extent to which the successive combination must be carried.*

From what precedes it would appear that the perception of perspectiveness in a binocular resultant or in a physical line requires a successive direction of the axes to contiguous points along the line. To what extent such successive vision must be carried in order to create a definite perception of perspectiveness is a question of much interest and difficulty.

According to the view of Sir David Brewster no part of the object is seen single and distinctly but that to which the optic

axes are for the moment directed and "the unity of the perception is obtained by the rapid survey which the eye takes of every part of the object," (Phil. Mag., May and June, 1844). On this theory the conditions of binocular vision of a perspective line would be as follows:

1. The perception of a perspective resultant line in the stereoscope would require the optic axes to be successively directed so as to unite every pair of corresponding points of the two component lines of the diagram, or which amounts to the same thing, they should be successively converged to every point of the perspective resultant.

2. This resultant would of necessity be perceived as a single perspective line the locus of all the resultant points.

3. In the case of two intersecting lines appearing instead of this resultant, these lines should neither of them have a perspective position.

But these conditions differ in important features from the phenomena of the perspective resultant before described. In the first place the perspectiveness of the resultant, although not perceived when the axes are steadily fixed at any one convergence, appears as soon as *the varying convergence has united contiguous points* of the component lines, and it affects the whole extent of the resultant, although the observer may have restricted the successive union to only a small fraction of the length of those lines. In the second place the perceived resultant instead of being a single line, is in most cases composed of two intersecting perspective lines varying their point of intersection and their degree of relief. The same conditions, as we have seen, apply to a physical line held in the position of the binocular resultant.

It would then appear that the perception of a perspective resultant line or a physical line in the same altitude, does not require the successive convergence of the axes to every point. The direction and amount of the relief may be sufficiently suggested by the view of a few contiguous points. The presence of two apparently or nearly coincident images would not of itself convey the idea of one or of two *perspective lines*, but if, along with this, we have even through a short distance the impression of an increasing or diminishing convergence and a change in the united direction of the axes, the optical signs of perspectiveness may be regarded as sufficiently complete to give us the perception either of two intersecting perspective lines of variable relief or that of a single line with the maximum relief.

When the line is of considerable length, we assure ourselves of its straightness and the degree of its relief by directing the eyes successively to all parts of it, but when it is short the images in the two eyes are distinct enough throughout to make a perception of the whole line definite and clear, without more than a very

partial survey, and this seems to be all that we employ in the ordinary vision either of a physical line or a binocular resultant in a perspective position.

From what has now been said, it is apparent that the process by which we see objects in relief by the binocular combination of drawings, is *nearly* identical with that by which we see the objects themselves, but I cannot agree with Sir David Brewster that "it is the *very same* process as in the vision of real objects in relief by the ordinary agency of our two eyes." It is true that as regards the successive change of convergency of the axes the action of the eyes is precisely the same whether they are directed to different parts of the binocular resultant or of the real object which it represents; but as formerly remarked it does not appear that the change of convergency is accompanied in the former case by any alteration of refractive power, while in the latter case we know that every increase or diminution of the one is associated with a corresponding change of the other.

18. *Of binocular direction.*

If on either stage of the stereoscope we place a diagram consisting of two dots or of two equal parallel lines, and after directing the eyes upon them so as to see them where and as they are, we then by the usual effort cause them to unite into one, we perceive that in their approach they move towards an intermediate position, and that the resultant dot or line lies in a direction midway between those in which the eyes previously saw the two separate objects. The peculiar sense of direction of which we are conscious in this and similar cases is evidently a resultant impression, derived from the co-existence or union of two different feelings of direction furnished by the two eyes. It may therefore I think, be not inappropriately called the *binocular direction*.

That in our ordinary vision with two eyes we have a similar sense of the binocular direction of objects is I think, proved by a variety of familiar facts. Thus for example, in aiming to strike an object with a stone or javelin, or in shooting at a mark with a bow or a gun held up to the breast, we endeavor to conform the motion which we impress to the feeling of direction derived from the combined use of the two eyes, and in the game of bowls it is this which guides the hand from which the ball is discharged.

The importance of this peculiar sense of direction, as distinguished from that of the eyes used singly, becomes more striking when we consider it in connexion with the perspective resultant formed by binocular combination, as in the following examples: In each case the combination is supposed to be made on the upper stage of the stereoscope, and, for the sake of clearness, the resultant is represented by a dotted line, the arrow head marking the nearer or upper extremity.

In viewing the perspective resultant CD of the lines A and B (fig. 29), each of these lines seems to occupy the position of the resultant, that is, each appears to lie in a direction midway between the two directions in which the components are seen without binocular combination. So when the resultant has the form of two intersecting nearly united perspective lines, each of them appears in a position nearly midway between those of the components. In this experiment the place of the resultant, that is the binocular place of both A and B, is in the vertical mesial plane of view, while that of A seen by the left eye singly is an oblique plane diverging to the left, and that of B seen by the right eye singly is in a similar plane diverging to the right.

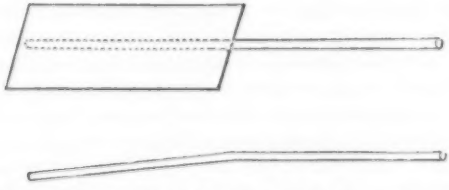
29.

By covering or omitting a part of one of the components as in fig. 30, and combining them by the usual binocular effort, we have a resultant consisting of a perspective line ED due to the union of A and the corresponding part of D, and a line EC not perspective but parallel to B and equal to the residuary part of it. Here by the binocular combination the upper half of B is made to appear in a direction different from the lower half not thus combined and therefore different from the direction in which it would itself appear independently of the combination.

30.

That the same influence operates in ordinary vision with both eyes is shewn by the following experiment. (Fig. 31.) Let

31.



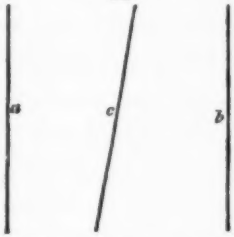
a line or thin rod be held at the limit of distinct vision in a perspective position between the two eyes, while by a card attached laterally we hide the near half of the rod from the right eye. If now we direct both eyes upon the rod, it will appear not as a straight line but as composed of two parts making an angle in the middle, the remote half lying in the mesial direction of our view, and the near half deflected a little to the right. The re-

sult of this simple experiment would be inferred from what has been said under a previous head, but it is so striking as at first to create some surprise.

Since the binocular direction of an object is not that in which it appears to either eye separately, it follows that two lines having similar attitudes to the two eyes respectively, may be made to appear in very dissimilar positions by combining a third line with one of them and not with the other.

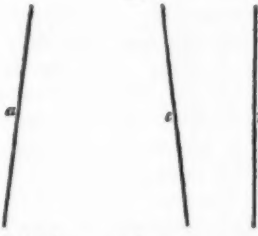
Thus if we place fig. 32 on the upper stage so that *a* shall be opposite the left and *b* and *c* opposite the right eye, and if then we combine *c* with *a*, we shall have a perspective resultant (fig. 33) lying obliquely on the left of *b*. Now this oblique resultant is the binocular place of *a* as well as of *c*, and thus the binocular combination has caused *a* which is really parallel to *b* and like *b* in a vertical visual plane to appear in an oblique visual plane receding from that of *b* downwards towards the left.

32.



33.

34.

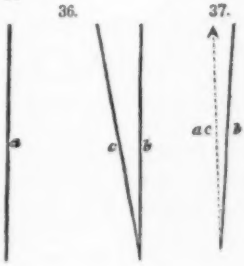


35.

Using fig. 34 in which *a* and *c* are equally and oppositely inclined to the vertical, and placing *a* opposite the left, and *b* and *c* opposite the right eye, we have, on combining *a* with *c*, a resultant lying in the mesial visual plane, (fig. 35). Here the binocular position of *a* and *c* is in a vertical visual plane parallel to that of *b*, and when projected binocularly on the plane of the paper, will appear to have a position parallel to *b*.

With lines related as in fig. 36, the effect is still more interesting. Here *a* and *c* when united form a perspective line which springs from the plane of the paper at the lower end of *b* and ascends obliquely towards the left (fig. 37). If in effecting the combination we look towards the lower ends of the lines where *b* and *c* are equidistant from *a*, a pause in the convergence will unite *a*

36.



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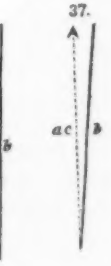


and *b* while *c* appears in the same plane and at the same inclination as in the diagram. But the moment the convergence increases, *a* and *c* unite and the perspective resultant rises from the plane of the paper.

Now in effecting the binocular union while looking towards the lower end of the lines, it is evident that for a moment *a* and *b* both form their images centrally on the retinal surface of the two eyes, and therefore according to the received law of single vision by corresponding points they might be expected to appear as a single line, that is to say, in the same place. That this coincidence does not occur is explained by the fact that *a* is seen in the binocular direction and that this direction being compounded of the impression made by *a* and *c* cannot be the same as that due to *b* alone or to *a* and *b* combined. Referring more directly to the visual actions concerned, we would say that in virtue of changing convergence the perspective idea becomes in this case associated with *a* and *c*, but no such idea is attached to *b*. Hence even when the images of *a* and *b* fall on corresponding points of the retinas, the conditions under which they are perceived are different and the two lines should not be expected to appear in the same place and direction.

The fact here demonstrated, that the images formed on corresponding points of the eyes may yet be perceived in separate places, is certainly at variance with the law of corresponding points, as usually expressed. *The contradiction however disappears, when, as is obviously proper, we restrict the application of the law to the cases in which the images are neither of them disturbed by combination with a third image.*

Another feature of the experiment worthy of especial notice is the fact that although the images of *a* and *b* are always formed on the retinas in parallel positions, no matter to what point of the perspective resultant we may for the moment direct our eyes, yet we never perceive them as parallel. The explanation is obvious. We perceive *a* in the binocular direction, that is, in the position of the resultant formed by the union of *a* and *c*, and therefore in an oblique plane, while we see *b* only in its natural vertical position.

19. *Experiment of Prof. Wheatstone to disprove the theory of corresponding points.*

Prof. Wheatstone adduces the following experiment to show "that similar pictures falling on corresponding points of the two retinæ may appear double and in different places."

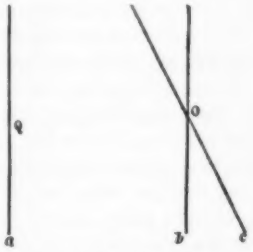
"Present in the stereoscope to the right eye a vertical line and to the left eye a line inclined some degrees from the perpendicular; the observer will then perceive, as formerly explained, a line the extremities of which appear at different distances before the eyes. Draw on the left hand figure a faint vertical line exactly corres-

ponding in position and length to that presented to the right eye and let the two lines of this left hand figure intersect each other at their centres. Looking now at these two drawings in the stereoscope the two strong lines each seen by a different eye will coincide and the resultant perspective line will appear to occupy the same place as before; but the faint line which now falls on a line of the left retina which corresponds with the line of the right retina on which one of the coinciding strong lines, viz., the vertical one, falls, appears in a different place." The author adds, "This experiment affords another proof that there is no necessary physiological connection between the corresponding points of the two retinas,"* &c.

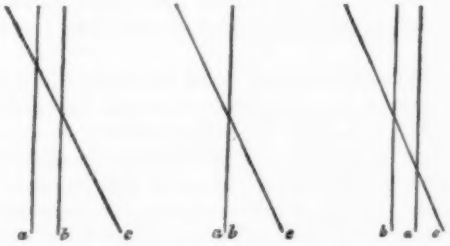
In this experiment the visual action and the consequent perception are analogous to those above described, and the explanation of the result is in all respects the same. As however too general a conclusion, has I think been drawn from it by the ingenious author, and as it has been the subject of an entirely different interpretation by Sir D. Brewster, I propose to consider it more in detail.

Using a drawing of the proportions represented in fig. 38, and placing it on the upper stage of the stereoscope so that *a* may be opposite the left and *b* opposite the right eye, it is easy by a quick change of convergence to combine *a* with *c*. The perspective resultant thus formed is seen intersecting the plane of the diagram and the line *b* in the point O. If we carefully graduate the convergence so as to mark the different stages of the phenomenon we may bring *a* to touch the upper end of *c* then to cross it successively nearer the middle, then to coincide with *b* then to cross on the other side of the centre, and lastly to touch the lower end of *c*. When this is done slowly the line *a* is seen in the various positions represented in fig. 39, always parallel to *b*.

38.



39.



* Phil. Trans., 1838, Part 2, or Phil. Mag., April, 1852.

If however we produce the intersections in somewhat quick succession, we have a perception more or less distinct of the perspectiveness of each of these intersecting lines, while *a* and *c* tending to coalesce causes the former to lose its parallelism to *b*. When the perspective effect is fully developed we perceive *a* and *c* only as they are united in the resultant. The axial movement has connected the sense of perspectiveness with each of them causing each to be perceived in the *binocular direction*.

Now it must be remembered that this perspective position and binocular direction being the result of successive combination of points in the two lines, is produced as well when the contiguous points thus united lie near the centre as when near the ends of the lines. But in the former case, that is, where the points about O in *c* are united with those about O in *a*, the line *b* must form its image centrally in the right eye when *a* forms its image in like position in the left. In these circumstances therefore the retinal images of *a* and *b* fall on corresponding points, at the same time that the former line is perceived in the binocular perspective position intersecting but not coinciding with the latter. In this momentary phasis of the experiment, it is obvious that the result is at variance with the law of corresponding points. When however the parts of *a* and *c* which the eyes successively combine are not near the centres of these lines, the image of *b* will be formed *away from the retinal centre* of the right eye, while that of the combining points of *c* will be in that centre, and the image of *a will be in the retinal centre* of the left eye. In this case the law of corresponding points requires that *a* and *b* should appear distinct from one another as they really do. Thus therefore *it is only when the resultant perspective is formed by combining the central points of the component lines that this experiment contradicts the law in question.*

But the characteristic feature in this class of effects is not so much the departure in certain conditions from the theory of corresponding points, as the general fact of which this is but a particular instance, viz., *that each of the lines binocularly combined is by virtue of the combination perceived in a position different from that in which it is seen without such combination, although in both cases the image of the line has the same retinal position in the same eye.*

In explaining the experiment of Wheatsone, above referred to, Sir David Brewster, if I rightly understand his remarks, (Phil. Mag., June, 1844,) ascribes the disappearance of *a* as a vertical line to an actual invisibility of the vertical image, or part of it, caused by its proximity on the retina to the image of the other component line. Of the fitful vanishing and reappearing of parts of the figure long since noticed in such cases by this eminent observer, I am abundantly satisfied from my own experience; but

I cannot see in the phenomenon, even were it of constant occurrence, an explanation of the appearances in question. Such a view seems to me inconsistent with the fact always observable in these combinations when not too rapidly made, that the vertical and other component lines assume a perspective attitude while still intersecting one another at an appreciable angle, that in this position the former has already lost its appearance of parallelism to the other vertical, that the two intersecting perspective lines in spite of an occasional disappearance in part, continue each generally visible until they have so closed upon one another as to appear coincident, and that even when thus united *the presence of both of them in the resultant is shewn by the continued visibility of the dots or other marks intended to distinguish them.*

In this experiment, various special phases of combination between the lines *a* and *c* present themselves, according to the direction and range of the axial movement by which the vision is effected. Sometimes the upper halves only, and sometimes the lower halves of the lines, are united, presenting an effect identical with that of fig. 36, previously described. But these and other features of the experiment being unessential to the present inquiry, need not be considered in this place. They will be treated of hereafter in connexion with a variety of analogous phenomena, belonging to the class of alternating or changeable combinations.

20. *Combination of lines lying in the same horizontal direction.*

When two *equal* right lines, fig. 40, lying in the same horizon-

40.

tal direction at right angles to the line of view are united binocularly either behind or in front of their real position they form, as might be expected, a resultant parallel to the components. The same convergence which serves to combine any two corresponding extremities as A and *a* causes also a union of the other ends B and *b* and combines all the parts of corresponding points of the two lines. When therefore the lines are so short as to be distinctly visible throughout their length without a change of axial direction, the formation of the resultant behind or before the components and parallel to them must be regarded as simultaneous throughout.

In the case of *unequal horizontal lines* the results are very different. Here the combination is to a certain extent successive involving a change of convergency and giving rise to a perspective resultant.

Sir David Brewster, in discussing the question of the coincidence of unequal figures adduces the first observatiou on this sub-

ject of which I have seen any description. "The best way," he says, "to make this experiment is to take two lines AB *ab*, (fig. 41,) of unequal lengths, and with a large pin to perforate

41.

the lines at AB *ab* so that when we attempt to unite them we shall see with perfect distinctness their four luminous extremities. When the point *a* is made to pass into A, I have never succeeded in making *b* pass into B. Whenever there is an appearance of this either turn round the paper or the head so as to separate the lines and it will be invariably seen that if *a* springs out of A, *b* will spring out of a point between A and B. The apparent coincidence therefore of AB with *ab* when it is seen arises from a *disappearance* of one or other of the extremities of the two lines."

In repeating this experiment in various forms, I have observed the effects described by Sir D. Brewster, but in addition to these I have obtained other results, which furnish a different and I think more satisfactory explanation of the optical coincidence of the unequal lines. Drawing the two lines in very opaque ink on somewhat thin letter paper, of the lengths of one inch and eight-tenths of an inch respectively, with perforations at the ends, I place the paper at the distance of distinct vision between the eyes and a bright window or the globe of a solar lamp, and by the usual binocular effort bring the two lines together—either behind or in front of their true place.

At first, if the axial movement be slow, I see the two luminous ends of the shorter line vibrating on the longer one, and if I bring *a* and A to coincide I see *b* and B separated, or making the latter coincide I see *a* and A separated, and all this time the two lines appear to lie in the plane of the diagram. But carrying the eyes from one end of the resultant to the other and uniting the the two ends in succession by a quick change of convergence, I observe the resultant suddenly assume *a perspective position.*

In my own case, this effect usually presents itself almost immediately, but persons unpractised in such experiments perceive at first only a confused impression, which, after they become accustomed to the requisite axial movements, gives place to a clear perception of the perspective line. The direction of the relief is of course dependent on the place at which the binocular union is effected as compared with the actual distance of the diagram. When the eyes are converged beyond the paper, the extremities *b* and B are united at a point nearer the observer than are *a* and A, and therefore the resultant recedes towards the left. When the axes meet in front of the paper, *b* and B are united at a greater distance than *a* and A, and hence the resultant recedes towards the right.

In this experiment the resultant appears as nearly single as in the case of two slightly inclined lines (p. 212) so that unless we pause to fix the gaze on one of the ends we cannot observe a want of coincidence at the other extremity. The moment however we make such a pause, the two luminous points of the other end separate, that of the short line moving on the long one to a distance equal to the difference of their lengths, and at the same time the resultant subsides from its perspective position.

When the coincidence of the two lines is such as to present the perspective effect, I cannot observe the disappearance of one or other extremity as mentioned by Sir D. Brewster. As a test of this I place a small dot on the outer edge of the perforations at A and *b* and I find that these marks always remain visible while the resultant is seen in perspective. I may add that such a disappearance would not explain the seeming coincidence of the unequal lines in a *perspective* resultant, but that on the contrary the formation of this resultant, as in the case of mutually inclined lines requires the actual though not simultaneous *union of the corresponding ends.*

The effects above described are readily obtained by using lines marked at the ends by short cross lines or by dots placed below or above them, and effecting the combination either without a stereoscope or by placing the figure on the upper or lower stage.

42.

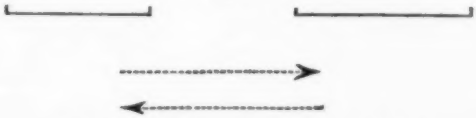


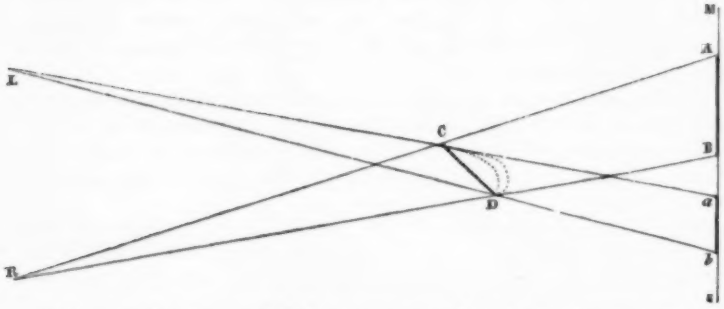
Fig. 42 represents the proportions of the lines which I have found convenient for either of these methods of operating. With this I obtain at once or very promptly, the binocular coincidence and the perspective effect. The dotted lines below denote the perspective directions, the arrow heads marking the nearer ends of the resultants. The upper of these indicates the resultant when the lines are united by using the upper stage or by looking beyond the paper, the lower when they are united by the lower stage, or at a plane nearer than the paper.

When the lines are not, as in the preceding experiments, strongly marked at the ends, the effect of the combination is variable, presenting sometimes a partial superposition of one line or the other without relief and sometimes a perspective resultant; I find however that by using broad lines strongly contrasting with the paper, I can in general obtain the latter result quite promptly, but the effect is more fluctuating and difficult to maintain than when the ends are conspicuously marked.

It is to observed that in all these experiments the perspective effect is directly dependant on the successive union of the cor-

responding *ends* of the lines, and that the two resultant points or short vertical lines thus formed are produced in the same way as the two resultants of unequally distant verticals treated of under a preceding head. The intervening points of the horizontal lines being all alike are not, as in the case of the extremities, marked out in corresponding pairs, and therefore may be combined binocularly in various ways, according to the rate of variation of the axial convergence in the interval between the terminal combinations. On a close examination of the resultant, I find accordingly, that while the actual amount of its relief remains constant, the intermediate parts of the line do not always appear in a straight direction between the near and remote end. Thus supposing the combination to be made in front of the paper MN (fig. 43) and that C and D represent the points at which the corres-

43.

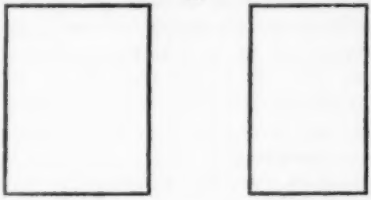


ponding ends A*a* and B*b* appear combined, I observe sometimes that the resultant loses the clear and satisfactory character of the straight perspective line CD, and appears vaguely as a curve or broken line retreating from the former towards the plane of the paper, but always terminating in C and D. The usual and more permanent form CD is obviously due to a uniform change of convergence in passing from C to D; the other more vague and evanescent appearances are I think produced by variable converging movements in the same interval.

From these experiments, we are I think entitled to conclude that *unequal horizontal lines not greatly differing in length may be combined so as to form a perspective resultant, the right and left ends of which are at unequal distances from the observer.*

This result, which appears to have escaped the attention of preceding experimenters, is of importance from the light thrown by it on the binocular union of unequal figures. In this connexion it will be referred to under a future head, but in the mean time the following example of its application to this question may properly conclude the present division of our subject.

44.



On combining the two rectangles (fig. 44), either without a stereoscope or by using the upper or lower stage of the sliding instrument, I find that the resulting figure always presents itself *in a perspective position*, of which the near vertical edge, as might be expected, appears shorter than the remote one. When the most perfect and steady effect is obtained the upper and lower edges appear as straight lines and the whole perspective figure seeming to be a plane. But sometimes in beginning the observation and occasionally when the eyes have grown fatigued, these lines assume the concave direction mentioned above, and the resultant figure has the aspect of a bent or warped surface. I may add that the perspective effect is clearly seen when the same figures are combined by means of one of Wheatstone's or Brewster's stereoscopes.

Note.—In the preceding No. on page 216, the right-hand figure of the twin diagram 25, should have been reversed as in 26.

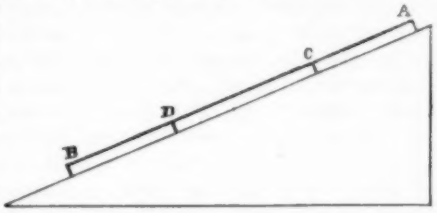
(To be continued.)

Art. XXIX.—*Remarks on the Rev. Henry Moseley's Paper, "On the Descent of Glaciers;"* by John LeConte, M.D., Professor of Nat. Philos. and Chem. in the Univ. of Georgia.

The Abstract of the Proceedings of the Royal Society for April the 19th 1855, published in the "Philosophical Magazine" for July, 1855, (4th Series, vol. x, p. 60 et seq.,) contains a very ingenious communication from the Rev. Henry Moseley. In this paper it is shown, by the application of established mechanical principles, that a solid body resting upon an inclined plane, whose angle of inclination is *less* than the limiting angle of resistance, will gradually *descend* by means of the *elongations and contractions incident to the ordinary changes of temperature.* To render this intelligible to those who may not have access to the paper of Mr. Moseley, I add the following condensed account of his investigation.

If we conceive two equal bodies (cubes for instance) of the same material, connected by a metallic rod, to be placed on an inclined plane (one being lower than the other), then when, by the increased temperature of the intervening rod, its tendency to extend becomes sufficient to push the *lower* of the two bodies *downwards*, it will not have become sufficient to push the *higher upwards.* The effect of its extension will, therefore, be to cause the lower of the two bodies to descend whilst the higher remains at rest. The converse of this will result from contraction: for

when the contractile force becomes sufficient to pull the upper body down the plane, it will not have become sufficient to pull the lower up it. Thus, in the contraction of the intervening rod, the lower body will remain at rest whilst the upper descends. It follows, therefore, that if the connecting rod is heated and then allowed to resume its former temperature, both of the bodies will descend the plane by an amount equal to the whole linear elongation of the rod. This will be repeated as often as there are oscillations of temperature, until step by step, they reach the bottom.



Next, suppose the uniform bar AB placed on an inclined plane, and subject to extension from increase of temperature ;—a portion CB will descend, and the rest CA will ascend ; the point C where they separate being determined by the condition, that the force requisite to push CA up the plane is equal to that required to push CB down it. When contraction takes place, the converse of the above will be true. The separating point D will be such, that the force requisite to pull DB up the plane is equal to that required to pull DA down it. DB is obviously, in this case, equal to CA in the other. Under these conditions, the determination of the lengths of the parts CA=DB and CB=DA, becomes a simple mechanical problem, from which Mr. Moseley deduces the formula,

$$x=\tfrac{1}{2}\mathrm{L}\left(1-\frac{\tan i}{\tan a}\right),\ (1),\ \text{in which,}$$

x=CA=DB
L=length of rod AB
i=angle of inclination of plane
a=limiting angle of resistance.

Let c be the elongation per linear unit under any *given* increase of temperature ; then the distance which the point B will be made to *descend* by this expansion,

$$=c(\mathrm{CB})$$
$$=c(\mathrm{L}-x)$$
$$\text{By (1)}\qquad =\tfrac{1}{2}\mathrm{L}c\left(1+\frac{\tan i}{\tan a}\right)$$

If we conceive the bar now to return to its former temperature, contracting by the same amount (c) per linear unit ; then the point B will by this contraction be made to *ascend* through the space

$$=c(\mathrm{DB})=cx$$
$$\tfrac{1}{2}=\mathrm{L}c\left(1-\frac{\tan i}{\tan a}\right)$$

The *total* descent (d) of the point B, by the elongation and contraction, being obviously equal to the *excess* of its *descent* above its *ascent*, is, therefore, determined by the equation

$$d=\tfrac{1}{2}\mathrm{L}c\left(1+\frac{\tan i}{\tan a}\right)-\tfrac{1}{2}\mathrm{L}c\left(1-\frac{\tan i}{\tan a}\right)$$

$$\therefore d=\mathrm{L}c\,.\,\frac{\tan i}{\tan a}\cdot \quad (2)$$

His attention having been first drawn to the influence of variations in temperature to cause the descent of a sheet of metal resting on an inclined plane, by observing that a portion of the lead which covers the south side of the choir of the Bristol Cathedral, which had not been properly fastened, had descended bodily eighteen inches into the gutter; Mr. Moseley proceeds to show, by the application of the foregoing formula (2), that the cause assigned is adequate to account for the fact observed. Thus far Mr. Moseley's reasoning is perfectly satisfactory and in accordance with what might be expected from one of the clearest and best writers on mechanical philosophy: but when he attempts to apply these facts and principles to the solution of the mechanical problem of the motion of glaciers, the *physical aspect* of his reasoning is singularly exceptionable. It is to this portion of the paper, that the attention of the reader is particularly directed.

After referring to the recent experiments of MM. Schumacher, Pohrt and Moritz, which show that the linear expansion of ice for a given increment of temperature, is greater than that of any other solid body hitherto examined; Mr. Moseley remarks, "Ice, therefore, has nearly twice the expansibility of lead, so that a sheet of ice would, under similar circumstances, have descended a plane similarly inclined, twice the distance that the sheet of lead, referred to in the preceding article, descended. Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses, by variations in the quantity and temperature of the water, which, flowing from the surface, everywhere percolates them. That they must, from this cause, descend into the valleys, is therefore, certain." (p. 64.) To show the adequacy of this cause, he applies the formula (2) to the Glacier de Léchaud, the Glacier du Géant, and the Mer de Glace. From the data in relation to length and inclination, furnished by the measurements of Prof. J. D. Forbes and others, assuming the mean *daily* variation of temperature to be 5°·321 Centigrade,* and the linear expansion of ice for an interval of 100° Centigrade to be

* This is the mean daily range of temperature, according to the observations of De Saussure, in the month of July, at the Col du Géant; and, of course, has reference to the *air* and *not* to the mass of the glacier.

0·00524,—he obtains, by the formula (2), numerical results, relative to the daily motion, sufficiently accordant (considering the nature of the problem) with those given by observation.

Without stopping to investigate whether a mass of matter having the structure and internal constitution of the glacial ice, would supply the same *mechanical conditions* as a metallic sheet or bar; it seems to me, that, whilst Mr. Moseley's reasoning may be perfectly applicable to ordinary cases of solid bodies resting upon inclined planes, its application as a cause of *Glacial motion* is peculiarly unfortunate. There are *physical considerations* which render this cause *totally inoperative* in the case of glaciers.

1. His theory is founded on a mistake as to a *physical fact*, arising from a strange oversight of the peculiar laws of *latent heat*. It is not a *fact*, that "variations in the quantity and temperature of the water," which percolates from the surface of the glaciers throughout their entire masses, would subject them to corresponding *variations of internal temperature*. For the water cannot permeate the capillary fissures without *melting* the ice, until its temperature is reduced to 32° Fahr. In other words, it is impossible for water to remain in contact with ice, *and both of them retain their respective physical states*, unless they are both at the temperature of melting ice. If warm water begins to descend, it *melts* the superficial ice and its temperature is speedily reduced to 32° Fahr. If the interior mass of ice is *below* the freezing point the water will *solidify*, and thus arrest the circulation:—so that, under any point of view, it appears to be physically impossible for the percolating water to convey the atmospheric variations of temperature into the interior of the glacial masses.

2. It is an immediate consequence of the laws of latent heat, referred to in the preceding paragraph, that ice *does not* change its temperature excepting for variations of temperature *below* 32° Fahr. Hence, during the *warm months*, the temperature of the glaciers (even at their surfaces) remains *invariably* at 32° Fahr., through all the fluctuations of atmospheric temperature: a conclusion abundantly corroborated by observation. And yet, experience shows, that their motion is *most rapid* during the *thawing season*:—that is, when their temperature must be *absolutely invariable!*

3. It appears very clearly from the thermometrical experiments of Prof. Agassiz, and from the observations of Prof. J. D. Forbes, that from 28° to 32° Fahr., is the habitual temperature of the great mass of a glacier; that the most rigorous nights in winter propagate an intense cold to but a very small depth; and that the temperature of the interior and lower parts is not sensibly lowered, even by the prolonged cold of winter. Hence it follows, that even during the *winter*, the cause which the theory demands (variations of temperature), is practically inoperative, when consid-

ered in relation to the great *mass* of the glacier.* Moreover, it is only during the *cold season*, that the superficial stratum of ice participates in the atmospheric oscillations of temperature; and yet, this is the season in which the motion of the glacier is *least rapid!* By some strange fatality, when the mechanical *cause* which the theory demands is *least active*, the *effect*, for which it is to afford an explanation, is at its *maximum* and *vice versa*.

The reference to the experiment of Mr. Hopkins is equally unfortunate for Mr. Moseley's theory. The following is the experiment alluded to:—"He placed a mass of rough ice, confined by a square frame or bottomless box, upon a roughly-chiselled flagstone, which he then inclined at a small angle, and found that a slow but uniform motion was produced, even when it was placed at an inconsiderable slope." "This motion," continues Mr. Moseley, "which Mr. Hopkins attributed to the dissolution of the ice in contact with the stone, would, I apprehend have taken place if the mass had been lead instead of ice; and it would have been but about half as fast, because the linear expansion of lead is only about half that of ice." (p. 67).

It is true, that a mass of lead might gradually have moved down the surface of the stone, from the expansions and contractions due to variations of temperature; but, would its motion have been *uniform?* or would it have been perceptible from hour to hour? As regards the motion of the mass of ice in this experiment, it is impossible to ascribe it to a similar cause; for Mr. Hopkins expressly states, that the motion took place *only* during the *melting* of the ice,† and consequently, when its temperature, by virtue of the laws of change of state, must have been *constantly* at the freezing point of water. There can be no satisfactory solution of the mechanical problem of the motion of glaciers, which does not contemplate the peculiar laws of *latent heat*, as well as the structure and other mechanical conditions of the glacial masses.

Athens, Georgia, Aug. 20th, 1855.

* But even if,—contrary to all observation and to all just theories of the propagation of heat,—we suppose the cold of winter to penetrate to any considerable depth into the glaciers, the vast amount of *latent heat* which must be extricated by the solidification of the first portions of water which permeate the cold mass after the thawing season commences, would very soon elevate its temperature to the freezing point; after which, its temperature could undergo no variation from the percolation of water.

† Vide Phil. Mag., 3d Series, vol. xxvi, p. 4, 1845

Art. XXX.—*On the Crystallization of Platinum from Fusion;* by J. W. Mallet, Ph.D.

Having recently prepared some bichlorid of platinum as a reagent by dissolving scraps of platinum foil, wire, etc., in nitro-muriatic acid, I poured off the yet strongly acid solution before the whole of the metal had disappeared, and washed and dried the remaining scraps. Among them there were five or six small beads of platinum which had been melted off from the end of a wire (of about $\frac{1}{15}$th inch in diameter) by the oxyhydrogen blowpipe flame. I was surprised to observe that these globules, which were spherical and quite smooth and brilliant before the acid had acted upon them, afterwards presented distinct traces of crystallization, resembling to the eye the little polyhedral beads of phosphate of lead which are obtained by fusing that salt before the blowpipe.

Some of the minute faces were plane or nearly so, but most of them were slightly rounded like those of many crystals of diamond. They presented for the most part the peculiar lustre of a metal with very minute striæ on the surface, but some of the facets were brilliant.

The prevailing form seemed to be the tetrakis-hexahedron, of which there was one very distinct example, and beside this, faces of the octahedron and *perhaps* of the cube with truncated angles—combinations of the cube with the octahedron—were recognisable on other beads.

Some of the globules were apparently *groups* of minute crystals, while one or two seemed to be distinct individuals.

The weight of the largest was not more than ·11 grm.

The crystalline faces were no doubt rendered visible by the *dissecting* action of the acid in the same way that the structure of a lump of alum may, as is well known, be brought out by partial solution in water—the chemically homogeneous mass offering at different points a greater or less resistance to the solvent, dependant upon the positions of these points with reference to the crystalline axes, just as the actual hardness of resistance offered to mechanical abrasion differs at different parts of the surface of a crystal.

The assumption of distinct crystalline structure by platinum under the circumstances mentioned, is remarkable from the extremely high melting point of the metal, the small quantity fused in each bead, and therefore the very short time in which each must have passed from the liquid to the solid state.

ART. XXXI.—*On an Index of Papers on subjects of Mathematical and Physical Science;* by Lieut. E. B. HUNT, Corps of Engineers, U. S. A.*

THE history of science exhibits a continual tendency towards specialisation. The sphere of each laborer becomes more and more limited as the area of research is expanded and distributed into well defined specialities. The same thing is observable in scientific research which in the mechanical and chemical arts is familiarized under the name of subdivision of labor. The same advantages and disadvantages of the specialising tendency are equally observable in the domains of science and of manufactures. The restriction of investigation and of industry to limited fields of exercise, has the effect to produce the highest skill within those fields, at the price of narrowness of conception and privation of power concerning all other spheres of action and research. The man whose life is spent in heading pins becomes almost preternaturally skillful in the manipulations of that manufacture, but this man in any other sphere of industry is a blunderer and a bigot. So too the man of science who assiduously cultivates a chosen speciality becomes therein preëminent, but in so doing, he is in great danger of losing his grasp on those generalities which transcend his particular field, and of becoming not only impotent but bigoted relative to those branches of research which he has not pursued. As the microscopist restricts his field, but intensifies his vision within that field, so the cultivation of a speciality withdraws into a single study the diffused powers of the original mind. The microscopist and the specialist find the same difficulty in seeing the parts in their relations to the whole. No problem is more important to the scientific investigator than that of rightly coördinating his general and special culture. If generalisation too largely prevail, his life will probably be exuberant but fruitless. If a speciality absorbs all his powers, there will be an abundance of ignoble and innutritious fruit. If he knows how properly to combine the general and the special, he is likely to bear such rich and lasting fruit as has made the names of Newton and Leibnitz, Euler and La Grange, Cuvier and Agassiz, and all of their illustrious kindred, such unfailing sources of strength. The power to generalise and the power to specialise must coëxist in the true magnate of science. Great things may be done by men strong in either; but not the greatest. Whoever neglects general culture in his eager pursuit of the special, harms his own nature. It may be said that power in specialities can only thus be attained; that specialist devotees

* Read before the American Association for the Advancement of Science, at Providence, August, 1855.

are essential to the progress of science; that science like another Juggernaut, demands the sacrifice of the man himself to his chosen pursuit. Whoever thus reasons has already sacrificed his humanity to his speciality, and is but shouting the praises of Brahma. Nor is it easy to adopt a theory of scientific castes, which would make of the specialists a lower and necessary grade, whose business it is to gather materials for the generalising noble to arrange in their true order, around some latent principle which he alone is privileged to see. That some minds are thus noble by nature and by training is certain enough, but it is hard to believe that many who could be of any assistance as specialists, cannot also do some part however small, as the generalizers of their own field and of its relations to others.

The present age of science may with great propriety be called the monographic age. We have reached that period when every subdivision of research needs to be presented in the form of a monograph. While it is still possible for the student of any single subdivision to pursue it in all the original papers treating thereon, it is quite impossible for the investigator of a more extended area to study the contained and related subjects in their whole range of original papers. To him it is quite essential that the various subdivisions of research should each be digested by thorough masters of their component contributions, and brought out in their orderly, well balanced proportions. The true, the proven, the mature, has need to be separated by adept monographers from the false, the speculative, and the crude. The contributions of many successive investigators, each tending to place their common speciality on higher ground, and to bring it out in more perfect definition, have all to be digested by one who is qualified to take the judicial point of view, and to use aright the privilege of moderns, who, as Bacon says, are the ancients of knowledge.

The model monograph is one which presents the known substantive facts of the subject treated in their natural order and relations with all attainable clearness, completeness and brevity, and which gives such an insight into the nature of all original memoirs thereon, as will enable investigators to recur to such originals as may be important for their special purposes. Already the mathematical and physical sciences have hundreds of subjects needing to be monographed, while comparatively few are yet so adequately presented under this form, as not to need either a new monograph or a revision of the old. The great number of investigators now at work; the late remarkable increase of periodical publications and memoirs devoted to scientific subjects; the subdividing or fissiparous tendency of modern science and its signal proclivity to annex fields of empyrical practice and make them rational or strictly scientific; these and other causes have thrown

nearly all the provinces included within the present domains of science, into the precise condition to need the services of well furnished monographers.

An invaluable result of such a presentation of these subjects would be found in the aid which investigators would thus receive, towards keeping up their general scientific culture, without turning too much away from their specialities. There seems no possible way by which investigators can maintain the general and the special in correct and harmonious relations, except by means of an ever-increasing expansion of their monographic aids. As the realm of knowledge goes on yearly expanding and steadily growing more and more to exceed the possible grasp of any one mind, this need of a direct and disembarrassed presentation of all positive results stated as far as possible in the common language of science, will become continually more pressing. It will be only by the aid of monographs of various degrees of generality, that the entire domain of science can henceforth be at all surveyed by any single investigator. The narrowest fields, being first duly digested into monographs, will furnish the materials for monographs of higher generality, and from these, step by step, the ascent will be to that universal monograph, which shall draw within its compass all the high generalities of that great epic cosmos in which science will attain its far off final consummation. When physical science becomes thus organized and digested, we shall no longer be constrained to behold the ardent mind, borne away by generalising speculations beyond all basis of fact, or the far more revolting arid mind, cramped within a petty speciality, and disdainfully ignoring all the vast domain of vital generalities, simply because it transcends its own dwarfed and grovelling comprehension.

The only worthy type of scientific mind is that which at the same time pursues special and general truth, which can see grand generalities underlying the minutest phenomena, and which can trace in the loftiest generalities the simple convergence of an infinite series of kindred special phenomena. The entire character of the active investigating mind of the coming generation will be much influenced by this monographic tendency and need. The extent to which we shall digest into consistent wholes the now dispersed and fragmentary materials on a long array of subjects in mathematical and physical science, will almost measure the prospective prevalence of that just union of the generalizing and the specializing power whence alone can spring a true progress in scientific philosophy.

The views now presented, are essential preliminaries to a full understanding of the bearings and workings of an index of papers on mathematical and physical science. This subject is one which the naturalists have better appreciated than the mathematicians

and physicists. The proof of this will be seen in that laborious work which the genius of Agassiz inaugurated and which presents, in four octavo volumes, the titles of all known papers on Natural History and Geology up to its date. These titles are arranged alphabetically under the names of their authors, as is probably most convenient in the departments included, Natural History and Geology being essentially descriptive sciences, are less fitted for the classification of the subjects included, than the more abstract or logical mathematical and physical sciences. A logical arrangement by subjects, at least for Natural History, will scarcely now be proposed by those interested, though it is possible that Engelman's Bibliography of Natural History which distributes authors alphabetically under a limited number of general heads, may yet serve as a model for indexing papers or memoirs in the same field.

In proceeding to examine somewhat the subject of an index of all original papers in mathematics and physics, it may be well briefly to state the circumstances which have led me to consider it. As an Assistant in the Coast Survey, I had on several occasions to make special investigations, in which it was desirable to examine all good relevant authorities and original memoirs. How to do this was the question. To range over series after series, index after index from beginning to end, would surely bring to light all such papers. But this is a labor of truly appalling magnitude and not to be thought of for each minor research. It only remained to start with such papers as I chanced to know of, or could find by the few indexes of series at my command. Then following up all the references which could be gleaned from these sources, I could go on till they were exhausted. By that time any man of moderate patience would himself be exhausted, and indisposed to beat up, for farther game. Yet what guarantee is there in this process that the very best papers may not entirely escape one's knowledge. I found from experience just this result; nor do I suppose that any person can be sure of having examined all those printed papers on a given subject of physical science, which are essential to a full comprehension of its history and development. He may find a vast deal more than he likes to read and yet leave the best of all unknown. This is peculiarly true of those just entering on a career of research. Veterans bear in their memories some traces of the leading papers published during their lifetimes, on all subjects even likely to enlist their activity. But the neophyte has no such aid, and does not even know how to get at the most accessible memoirs. A great loss of time in turning over leaves for the want of specific references is common alike to young and old. Thus for instance when I wished to find all the published descriptions of automatic tide guages, I spent a great deal of time to find what could be read in five minutes, and am by no

means confident that I have seen the best description of the best foreign guage. Every persons experience must abound in such illustrations.

I did not many times repeat this experience of time-sacrifice before seeing the advantage of making this search for materials a general one and so once for all mastering the forces which were likely to be needed. I was thus led to form the plan of a systematic examination of all the principal series of scientific memoirs and periodicals for the purpose of extracting such titles of papers as I judged proper for a special index on subjects directly related to the various Coast Survey operations. This project met the hearty approval of the Superintendent, who has doubtless experienced its need more than any other person. He authorized my proceeding with its execution and has furnished every encouragement. I have already examined considerably over 1000 volumes of Memoirs, Translations, Scientific periodicals, &c., and it is my expectation to advance this index so far as to permit its being included in the Coast Survey Report of this year as an Appendix. It will be impossible for me to exhaust the field, but fortunately a supplement can at any time be added with the contributions of succeeding years, in which omissions from the first index may be supplied. This index will be arranged by means of a detailed classification under subjects, and will embrace various heads of interest to cultivators of physical science at large. Though the principle of relation is that of probable use in the Coast Survey operations, the ground covered will be of considerable extent.

Such a fragment alone, though rather laborious, I should not think worthy to bring before this Association, were it not that it had served to indicate a far more catholic plan and one which if executed will prove a signal benefaction to all cultivators of mathematical and physical science. This plan is simply an extension of the one already defined, so as to arrange in one general repository, all titles of papers on mathematical and physical science. The ground occupied by the index of Agassiz and his collaborators should be included so as to aggregate in the two works, references to all scientific researches. Engineering, Machines, Chemical, Mechanical and Artistic Technology would be invaluable additions, and should if possible all be embraced. All these titles being duly extracted, and when necessary annotated, would admit of classification into several volumes, each containing a connected group of subjects so that every investigator would find most of the references he would need in a single volume.

It was to somewhat such a shape that this plan reduced itself when I came to confer with Prof. Henry and Prof. Baird of the Smithsonian Institution. In the programme of objects proposed to be accomplished by that Institution is included a project of this

very character. Prof. Henry having as he declares found particular advantages from using the mathematical and physical index arranged by that clear-minded philosopher, Dr. Thomas Young, on a somewhat similar idea, was not likely to forget this among plans for the increase and diffusion of knowledge. It is indeed a plan for rendering accessible the knowledge already recorded, and as such eminently deserves the aid of that Institution. Were the work done and well done, the Institution would undoubtedly undertake its publication. But done it is not, and the question is how to accomplish its proper excution. Could I have commanded my own time, the performance of this work would have been a congenial labor. In spite of that precariousness of station which is my professional prerogative, I was much inclined to this undertaking and probably should have begun it, had not the assignment of triple public duties made it simple folly to venture it. Thus with the best intentions I am obliged to forego this expectation, and have brought the subject here in the hope that some one more favored by circumstances may be induced to undertake so needful an enterprise. I will gladly lend any aid in my power, to any one who is both fit and willing thus to work for the good of science. How far assistance and compensation might be allowed by the Smithsonian Institution in the execution of this plan is, of course, not for me to say. From the real value of the proposed work and the interest felt by the Smithsonian officers in its accomplishment, it is fair to infer that no reasonable aid would be denied which the means of the Institution would authorize. Its valuable collection of memoirs and transactions would be of peculiar value in this connection.

There is one excellent suggestion for which I am indebted to Prof. Baird. This is that an Index of American Scientific papers would be a useful and proper beginning for such an undertaking. The exceedingly scattered and anomalous vehicles through which American investigation have reached the present time, will make this portion of the search rather peculiar, and it is on this account much more needful. In truth, we do not know the real wealth of our own science, especially those of us who are young in such pursuits. Europe too is in a state of deplorable ignorance relative to our investigations, an ignorance which has considerable excuse too; for how can we expect foreigners to ferret out science from Patent Office and Coast Survey Reports or other public documents, Regents Reports, State Legislative documents, or indeed from any except the standard journals and memoir series. Thus there are very good reasons for an American Index, as preliminary to a general one, with which it could be regularly incorporated.

The work now proposed is certainly one of great labor. It will require several years and an examination of various libraries for its completion. Our own libraries will not offer all the

materials needed for its completion. English, French, German, Dutch, Spanish, Italian, Hungarian, Russian, Swedish and Latin series will need to be ransacked before the work is finished. A delicate exercise of judgment in accepting and rejecting titles will be required. There will be sundry questions such as these—Shall any anonymous papers be included? Shall papers in the library magazines be selected? Shall titles be given in full and literally, or cut down, modernized, translated or anotated? Shall translations be included? Shall reprints into more accessible series be referred to? &c. It has been my practice to include the date in each title, and to give the limiting pages of each memoir as an indication of the fullness with which its subject is treated.

The labor of classification will be one demanding a truly cosmopolite mind. Rightly to distribute the hundreds of subjects and always to maintain the truly logical catena of succession, will require a broad power of appreciation, such as few possess. Many plans of classification might be devised each having some advantages. But whatever system is adopted, an alphabetical index of subjects would make it easy of practical use. This study of classification would of itself be a most valuable research: for I am confident that any abstract or à priori plan would find the materials somewhat refractory and thus would undergo modification and amplification. The general alphabetical arrangement of titles by authors' names, would I think be quite unfit for this case. The greatest benefit of the whole plan would be in its bringing together references to all which has been written on each subject, and thus at once giving a clue to all the materials for perfect monographs. It is of far less importance in mathematics and physics to know the *who* than the *what*. But both can be sufficiently known by alphabetical arrangement of authors under subjects, and by an index of authors' names with references to all the subjects under which each has titles. Thus all the papers of any author and all the papers on any subject may be directly found.

I need not further enlarge on this plan. Should there be any who has capacity and courage to undertake the great enterprise indicated, farther hints would be superfluous for him. Should there be several who are willing to associate their labors, each taking certain series, and so together exploring the whole, it will not be difficult to concert such a general plan. The greatest difficulty of such an association would be in securing thorough coöperation and uniform execution. The means of classification would of course be by movable slips, and thus most incongruities of plan would be avoided by making the classification the work of one.

When we look at an individual labor so valuable as Poole's Index of Periodical Literature, we cannot doubt that the labor now proposed is destined ere long to enlist far greater industry and talent, and that if seriously undertaken it must succeed.

Dr. Young's Index, contained in the volumes of his works, would afford valuable aid towards conceiving the plan, though it is very far from perfect, and of too old a date to permit the continuance of its classification without much change. The proposed Index seems to be one of those undertakings which the current of events will render too indispensable not to be ere long begun. If so extended as to embrace Engineering, Machines, and the Technology of Art and Manufactures, (Chemistry in all its applications would of course be included,) it will become sufficiently valuable to many merely practical interests to enlist their active support. Our Patent Office might well afford to defray all the cost of such a work in those departments over which examinations for patents are required to be made.

If for a moment we conceive the result attained and the entire compass of reference to Mathematical and Physical papers brought into a systematic body, under specific subject-headings, we shall better realize its value. The course of investigation on any particular subject would be made simple and direct. By yearly supplements we might be kept informed of new papers beyond our ordinary range. The investigator would proceed to exhaust all the papers of value on any subject in hand, and would know when he was done; that he would start thoroughly furnished for making additions to existing knowledge instead of wasting his strength on work already done. The preparation of a monograph would no longer involve a chartless roaming over a boundless sea, but our materials could be used in certainty of their completeness and in such succession as our convenience might dictate. Monographs thus made easy could not fail to cover field after field with unprecedented completeness and facility. We should in each branch be soon furnished with that clear synoptic presentation of all its important elements and results which would enable us to give each speciality its true value and relations on the general chart of scientific coördination. Our general views would keep pace with our special investigations, and our minds would attain that harmony of culture, characteristic of the well developed man. Alike versed in those grand generalities which form the groundwork of creation, and practiced in the study of our chosen fields of research, we should steer clear of fruitless speculations and of the bigotry of petty knowledge. Such would be the tendencies of the work proposed; what now remains is simply to do it, and for this nothing is wanting but the man or men. The benefit is for science among all nations; the benefactory reward will be a truly honorable distinction and a consciousness of usefulness such as few living men can claim. When we remember how the mighty dead and the honored living have given record to their best thoughts, in hope that the world would not willingly let them die, it becomes in us a deed of pious duty to retrace and render legible the inscriptions on these too neglected record tablets.

Art. XXXII.—*On the Geographical Distribution of Crustacea;* by James D. Dana.

(Continued from page 178.)

In continuing the extracts from the writer's Report on Crustacea, the notes relating to the genera and species characterising the several provinces exhibiting their relations and distinctive characters may be here for the most part omitted. The following are a few facts of special interest.

The relations of the Mediterranean region to Japan are mentioned by De Haan. The genera strikingly Mediterranean which occur in Japan, are *Latreillia*, *Nika*, *Caridina*, *Ephyra*, *Sicyonia*, *Achæus*, *Pandalus*, *Lysmata;* and the species of the last three, together with *Squilla mantis*, are probably identical, viz., *Pandalus pristis*, *Lysmata seticaudata*, and *Achæus Cranchii*, which last is at least hardly distinguishable, according to De Haan, from the *A. japonicus*. *Portunus corrugatus* is also closely like a Japan species, according to De Haan. The *Cycloes* of the Canaries is another of the Atlantic species, allying the Atlantic region to Japan, as above mentioned. Doclea is also an Oriental genus, represented in the Occidental kingdom by Libinia. It has but one described species out of the Oriental kingdom.

DECAPODA COMMON TO THE CELTIC PROVINCE AND THE MEDITERRANEAN.*

1. *Brachyura.*

Maia squinado, A.
Pisa tetraodon, A.
" lanata (Gibsii), A.
Achæus Cranchii, A.
Stenorhynchus phalangium, A.
Eurynome aspera.
Perimela denticulata, A.
Xantho floridus, A.
" rivulosus, A.
Pilumnus hirtellus.
Portunus pusillus.
" Rondeletii, A.
" depurator (plicatus), A.
" marmoreus.
" corrugatus, A.
" holsatus.
Carcinus mænas, A.
Portumnus latipes, A.
Gonoplax angulata, A.
Goniograpsus varius, A.
Pinnothera pisum.
Thia polita.
Corystes dentatus.

2. *Anomoura.*

Dromia vulgaris, A.
Porcellana platycheles, A.
" longicornis, A.
Bernhardus Prideauxii, A.
" Forbesii.
" streblonyx.
Clibanarius oculatus.
Galathea strigosa, A.
" squamifera.

3. *Macroura.*

Callianassa subterranea.
Arctus ursus, A.
Palinurus vulgaris, A.
Homarus vulgaris, A.
Nephrops norvegicus.
Crangon fasciatus, A.
" vulgaris.
" cataphractus, A.
Nika edulis, A.
Alpheus ruber, A.
Athanas nitescens, A.
Hippolyte varians, A.
" viridis, A.
Palæmon serratus, A.
Pasiphæa sivado.
Penæus sulcatus (caramote), A.

* Those species that are reported by Lucas from Algiers, are followed by the letter A. They also occur elsewhere in the Mediterranean.

The genus Xantho, in *X. rivulosus* and *X. floridus* here reaches its extreme cold limit in the Celtic Province. *Nephrops norvegicus*, although more properly pertaining to the next province north, occurs also within the limits of this; and it has even been taken in the Mediterranean. *Stenorhynchus phalangium* and *Portunus pusillus*, reach south into the Mediterranean and north to the Frigid zone; *Portunus holsatus*, *Galathea strigosa*, and *Porcellana platycheles*, south to the Canaries and north into the subfrigid. * * *

Turning Cape Agulhas, we soon come into a zoological world widely different from that of the Atlantic coasts. The coast immediately east to longitude 30°, belongs still to the temperate zone, and constitutes a distinct province, which we call the Algoa Province (from Algoa Bay), the length of which, measured from Cape Agulhas, is full five hundred and fifty miles.

Passing beyond this, we reach the Natal province, and here we recognise at once the seas of India and the Pacific Ocean. Krauss mentions eighty-one Natal species of Podophthalmia, not thirty of which are peculiar to this region. *Twenty* are found in the Indian Ocean, *eighteen* in the Red Sea, *thirteen* in Japan, *eight* in Australia, *five* in the Isle of France, besides *three* European species, and *three* American. We observe further that, twenty-two of the species of Podophthalmia occur in the Pacific Islands, among which are four species supposed by Krauss to be peculiar to Natal, viz., *Pagurus* (*Clibanarius*, D.) *virescens*, Kr., *Pagurus* (*Calcinus*, D.) *elegans*, *Galene natalensis*, Kr., *Platyonychus* (*Kraussia*, D.) *rugulosus*, Kr., all of which occur at the Hawaiian Islands.*

Of the *European* species, one is the cosmopolite *Gonodactylus chiragrus*, Latr. The others are *Alpheus Edwardsii*, and *Gammarus pulex*, Fabr. *Megalopa mutica* and *Hippolyte ensiferus*, also reported from South Africa, do not occur at Port Natal. The *American* are the cosmopolites *Goniograpsus pictus*, and *Gonodactylus chiragrus*, together with *Eriphia gonagra*, Edw. The *Sesarma reticulata*, Say, and *Plagusia tomentosa*, Lk., also South African, are not from Port Natal.

The wide distribution of the same species over the Oriental seas, from the African coast through the Pacific Ocean, is well shown in the following tables—a distribution due to the wide range of the tropical and subtropical waters. The *African* section here includes the east coast of Africa, with the adjoining islands; the *Indian*, the East Indies, southeast and east coast of Asia, and Eastern Australia; the *Pacific*, the Pacific Ocean.†

* The *Galene hawaiensis*, D., is so closely like the *G. natalensis*, that we believe there is not sufficient reason for considering them distinct.

† I. Fr. stands for Isle of France; E. I. for East Indies; Haw. for Hawaiian Islands; Jap. for Japan; Nat. for Natal; Feej. for Feejees.

SPECIES COMMON TO THE THREE SECTIONS, THE AFRICAN, THE INDIAN, AND THE PACIFIC.

1. *Brachyura.*

Parthenope horrida.—I. Fr., Red Sea; E. I.; Haw.
Atergatis limbatus.—Red Sea; E. I.; Feej.
Atergatis floridus.—Natal; E. I.; Tonga, Paumotus; Tahiti.
Carpilius maculatus.—I. Fr.; E. I.; Jap Samoa, &c., to Paumotus.
Carpilius convexus.—R. Sea: E. I.; Jap.; Feej., Haw.
Actæa hirsutissima.—R. Sea; Samoa.
Chlorodius niger.—R. Sea (N.); E. I.; Feej., Tonga, Samoa.
Trapezia ferruginea.—R. Sea; E. I.; Pacific.
Cymo Adreossyi.—R. Sea; E. I.?; Samoa, Tahiti.
Scylla serrata.—Natal; R. Sea; E. I.; Jap.; Samoa.
Lupa sanguinolenta.—Nat.; I. Fr.; R. Sea; E. I.; Haw.
Thalamita admete.—Nat.; R. Sea; E. I.; Samoa, Wake's, Haw.
Thalamita crenata.—Nat.; R. Sea (S.); E. I., Jap., Feej.
Cleistostoma Boscii.—Nat.; R. Sea; [E. I.?]; Feej.
Podophthalmus vigil.—I. Fr.; E. I.; Jap.; Haw.
Ocypoda brevicornis.—I. Fr.; E. I.; Tonga.
Acanthopus planissimus.—Nat.; E. I.?; Samoa, Tahiti, Paumotu, Haw. [also Madeira.]
Calappa tuberculata.—Nat.; I. Fr.; R Sea; E. I.; Feej., Tonga, Haw.
Calappa fornicata.—I. Fr.; E. I.; Feej.

2. *Anomoura.*

Pagurus difformis.—I. Fr.; E. I.; Feej.
Pagurus punctulatus.—E. I.; Haw.
Calcinus tibicen.—Nat.; E. I.; Samoa: Wake's, Tahiti, Paumotus, Haw.
Calcinus elegans.—Nat.; E. I.?; Wake's, Paumotus, Haw.
Aniculus typicus.—I. Fr.; Jap.; Wake's, Paumotus.
Clibanarius virescens.—Nat.; E. I.; Feej.
Cenobita rugosa.—Nat.; E. I., Jap.; Feej.; Samoa, Tonga, Paumotus.
Birgus latro.—I. Fr.; E. I., Jap.; Samoa, Swain's, Paumotus.

3. *Macroura.*

Parribacus antarcticus.—I. Fr.; E. I.; Samoa, Paumotus.
Panulirus penecillatus.—R. Sea; E. I.; Pacific.
Hippolyte marmoratus.—E. I.; Pacific; Haw.
Stenopus hispidus.—I. Fr.; E. I.; Paumotus.

4. *Anomobranchiata.*

Pseudosquilla stylifera.—I. Fr.; Feej.; Haw.
Gonodactylus chiragrus.—Nat.; I. Fr., R. Sea; E. I.; Feej., Tonga.

Of the above species, a few occur in both the torrid and subtorrid regions of these three sections of the Oriental kingdom, that is, in the Erythrean, Natalensian, Indian, Liukiuan, Polynesian, and Hawaiian Provinces. These are:—*Lupa sanguinolenta*, *Podopthalmus vigil*, *Calappa tuberculata*, *Acanthopus planissimus*, *Calcinus tibicen*, *C. elegans*, and *Gonodactylus chiragrus*. *Grapsus pictus* is not included; it has not been reported from the eastern coast of Africa. The above list must be much increased as the species of the different regions are better understood. Some of the species have a range of over twelve thousand miles. Many species common to Natal and Japan or the Hawaiian Islands, are given in the above list. We add below a list of—

SPECIES COMMON TO THE NATAL AND THE LIUKIUAN (SOUTH JAPAN) OR HAWAIIAN PROVINCES OF THE SUBTORRID REGIONS, AND NOT YET OBSERVED IN THE TORRID REGION INTERMEDIATE.

Micippa thalia.—Nat. and Jap.
Xantho affinis, De H.—Nat. and Jap.
Xantho obtusus, De H.—Nat. and Jap.
Carpilius petræus, De H.—Nat, (I. Fr.) and Jap.
Charybdis granulatus.—Nat. and Jap.
Thalamita prymna.—Nat. and Jap.
Gelasimus arcuatus.—Nat. and Jap.
Gelasimus lacteus, De H.—Nat. and Jap.
Ocypoda cordimana.—Nat. and Jap.

Sesarma picta.—Nat. and Jap.
Sesarma affinis.—Nat. and Jap.
Kraussia rugulosa.—Nat. and Haw.
Galene natalensis.—Nat. and Haw.
Dromia hirsutissima.—S. Afr. and Haw.
Calappa spinosissima.—I. Fr. and Haw.
Doto sulcatus, Nat., Jap., and R. Sea.

The species of New Zealand, while somewhat related to those of southeastern Australia, have rather close relations to those of Chili, and also some resemblance to those of Britain. The genera *Ozius*, *Hemigrapsus* and *Chasmagnathus* are common to both New Zealand and the part of Australia referred to. The following genera characterize both Chili and North New Zealand:—*Cancer*, *Ozius*, *Cyclograpsus*, *Paguristes*, and *Betæus*; and the *Cancer Edwardsii* and *Plagusia tomentosa* appear to be common to the two provinces, while the genus Cancer is not elsewhere known out of America and Northern Europe. *Palæmon affinis* of the Bay of Islands, as Edwards observes, is hardly distinguishable from *P. squilla* of the coasts of France and Britain. The species of *Portunus* in these southern seas are representatives of the most characteristic of European genera, and they belong rather to the cold temperate than subtemperate regions of the Australian and New Zealand seas. *Portunus integrifrons* is reported from Tasmania (Van Diemens Land). Ozius represents Xantho of the British Channel.

The occurrence in New Zealand of this representative of a cold water *Xantho*, of the *Palæmon affinis* so near a European species, of *Cancer* and *Portunus*, which are found together only in British seas, shows a striking zoological relation between these antipodes—a relation plainly dependent on the similar insular character and oceanic temperature of the two regions, Britain and New Zealand.

In the north Polar seas, the species have often a wide range, and probably pass from one ocean to the other through the Polar oceans. Thus *Crangon boreas*, *Carcinas mænas*, *Pagurus streblonyx*, *Hippolyte aculeatus*, are not only found on opposite sides of the Atlantic, but also in the North Pacific.

Origin of the Geographical Distribution of Crustacea.

The origin of the existing distribution of species in this department of zoology deserves attentive consideration. Two great causes are admitted by all, and the important question is, how far the influence of each has extended. The first, is *original local creations*; the second, *migration*.

Under the first head, we may refer much that we have already said on the influence of temperature, and the restriction of species to particular temperature regions. It is not doubted that the species have been created in regions for which they are especially fitted; that their fitness for these regions involves an adaptation of structure thereto, and upon this adaptation, their

characteristics as species depend. These characteristics are of no climatal origin. They are the impress of the Creator's hand, when the species had their first existence in those regions calculated to respond to their necessities.

The following questions come up under this general head:—

1. Have there been local centres of creation, from which groups of species have gone forth by migration?

2. Have genera only and not species, or have species, been repeated by creation in distinct and distant regions?

3. How closely may we recognise in climatal and other physical conditions, the predisposing cause of the existence of specific genera or species?

With regard to the *second* head, migration, we should remember, that Crustacea are almost wholly maritime or marine; that marine waters are continuous the globe around; and that no sea-shore species in zoology are better fitted than crabs for migration. They may cling to any floating log and range the seas wherever the currents drift the rude craft, while the fish of the sea-shores will only wander over their accustomed haunts. Hence it is, that among the Pacific Islands the fishes of each group of islands are mostly peculiar to the group, while the Crustacea are much more generally diffused.

A direction and also a limit to this migration exist, (1) in the currents of the ocean, and (2) in the temperature of its different regions. Through the Torrid zone, the currents flow mainly *from the east* towards the west; yet they are reversed in some parts during a certain portion of the year. But this reversed current in the Pacific never reaches the American continent, and hence it could never promote migration to its shores. Again, beyond 30° or 35° of north or south latitude, the general course of the waters is *from the west*, and the currents are nearly uniform and constant. Here is a means of eastward migration in the middle and higher temperate regions. But the temperature regions in these latitudes are more numerous than in the tropics, and species might readily be wafted to uncongenial climates, which would be their destruction; in fact they could hardly escape this. Moreover, such seas are more boisterous than those nearer the equator. Again, these waters are almost entirely bare for very long distances, and not dotted closely with islands like the equatorial Pacific.

In the northern hemisphere, on the eastern coasts especially, there are warm currents from the south and cold currents from the north. The former overlie the latter to a great extent in the summer and may aid southern species in northward migrations. Cape Hatteras is nearly the termination of the summer line of 70° (see Maury's Chart), a temperature which belongs to the subtorrid region in winter. On the China coast, at Macao, there

is a temperature of 83° in July, and in the Yellow Sea, of 78° to 80°. But such northward migrations as are thus favored, are only for the season; the cold currents of the winter months destroy all such adventurers, except the individuals of some hardier species that belong to the seas or have a wide range in distribution. Sea-shore Crustacea are not in themselves migratory, and are thus unlike many species of fish. Even the swimming Portunidæ are not known voluntarily to change their latitudes with the season.

The following is a brief recapitulation of the more prominent facts bearing on these points.

1. The distribution of individuals of many species through twelve thousand miles in the Torrid zone of the Oriental seas.

2. The very sparing distribution of Oriental species in Occidental seas.

3. The almost total absence of Oriental species from the west coast of America.

4. The world-wide distribution within certain latitudes of the species we have called cosmopolites.

5. The occurrence of closely allied genera at the Hawaiian Islands and in the Japan seas.

6. The occurrence of the same subtorrid species at the Hawaiian Islands and at Port Natal, South Africa, and not in the Torrid zone intermediate, as Kraussia rugulosa and Galene natalensis.

7. The occurrence of identical species in the Japan seas and at Port Natal.

8. The occurrence of the same species (Plagusia tomentosa) in South Africa, New Zealand, and Valparaiso; and the occurrence of a second species (Cancer Edwardsii (?)) at New Zealand and Valparaiso.

9. The occurrence of closely allied species (as species of Amphoroidea and Ozius) in New South Wales and Chili.

10. The occurrence of the same species in the Japan seas and the Mediterranean, and of several identical genera.

11. The occurrence of a large number of identical species in the British seas and the Mediterranean; and also in these seas and about the Canary Islands.

12. The occurrence of closely allied, if not identical, species (as of Palæmon) in New Zealand and the British seas; and also of certain genera that are elsewhere peculiarly British, or common only to Britain and America.

13. An identity in certain species of Eastern and Western America.

The following are the conclusions to which we are led by the facts.—

I. The migration of species from island to island through the tropical Pacific and East Indies may be a possibility; and the same species may thus reach even to Port Natal in South Africa. The currents of the oceans favor it, the temperature of the waters is congenial through all this range, and the habits of many Crustacea, although they are not voluntarily migratory, seem to admit of it. The species which actually have so wide a range are not Maioids (which are to a considerable extent deep-water species), but those of the shores; and some, as *Thalamita admete*, are swimming species.

II. The fact, that very few of the Oriental species occur in the Occidental seas, may be explained on the same ground, by the barrier which the cold waters of Cape Horn and the South Atlantic present to the passage of tropical species around the Cape westward, or to their migration along the coasts.

Moreover, the diffusion of Pacific tropical species to the Western American coast is prevented, as already observed, by the westward direction of the tropical currents, and the cold waters that bathe the greater part of this coast.

III. When we compare the seas of Southern Japan and Port Natal and find species common to the two that are not now existing in the Indian Ocean or East Indies, we hesitate as to migration being a sufficient cause of the distribution. It may, however, be said that driftings of such species westward through the Indian Ocean may have occasionally taken place; but that only those individuals that were carried during the season quite through to the *subtorrid* region of the South Indian Ocean (Port Natal, etc.), survived and reproduced, the others, if continuing to live, soon running out under the excessive heat of the intermediate equatorial regions. That they would thus run out in many instances is beyond question; but whether this view will actually account for the resemblance in species pointed out is open to doubt.

IV. When further, we find an identity of species between the Hawaiian Islands and Port Natal—half the circumference of the globe, or twelve thousand miles, apart—and the species, as *Galene natalensis*, not a species found in any part of the torrid region, and represented by another species only in Japan, we may well question whether we can meet the difficulty by appealing to migration. It may however be said, that we are not as yet thoroughly acquainted with the species of the tropics, and that facts may hereafter be discovered that will favor this view. The identical species are of so peculiar a character that we deem this improbable.

V. The existence of the Plagusia tomentosa at the southern extremity of Africa, in New Zealand, and on the Chilian coasts, may perhaps be due to migration, and especially as it is a south-

ern species, and each of these localities is within the subtemperate region. We are not ready however to assert, that such journeys as this range of migration implies are possible. The oceanic currents of this region are in the right direction to carry the species eastward, except that there is no passage into this western current from Cape Horn, through the Lagullias current, which flows the other way. It appears to be rather a violent assumption that an individual or more of this species could reach the western current from the coast on which it might have lived; or could have survived the boisterons passage, and finally have had a safe landing on the foreign shore. The distance from New Zealand to South America is five thousand miles, and there is at present not an island between.

VI. Part of the difficulty in the way of a transfer of species between distant meridians might be overcome, if we could assume that the intermediate seas had been occupied by land or islands during any part of the recent epoch. In the case just alluded to, it is possible that such a chain of interrupted communication once had place; and this bare possibility weakens the force of the argument used above against migration. Yet as it is wholly an assumption, we cannot rely upon it for evidence that migration has actually taken place.

VII. The existence of the same species on the east and west coasts of America, affords another problem, which migration cannot meet, without sinking the isthmus of Darien or Central America, to afford a passage across. We know of no evidence whatever that this portion of the continent has been beneath the ocean during the recent epoch. An argument against such a supposition might be drawn from the very small number of species that are identical on the two sides, and the character of these species. *Libinia spinosa* occurs at Brazil and Chili, and has not been found in the West Indies. *Leptopodia sagittaria*, another Maioid, occurs at Valparaiso, the West Indies, and the Canaries.

VIII. The large number of similar species common to the Mediterranean and British seas may be due to migration, as there is a continuous line of coast and no intermediate temperature rendering such a transfer impossible; and the passage farther south to the Canaries of several of the species is not beyond what this cause might accomplish. Still, it cannot be asserted that in all instances the distribution here is owing to migration; nor will it be admitted unless other facts throw the weight of probability on that side.

IX. But when we find the same Temperate zone species occurring in distant provinces, these provinces having between them no water communication except through the Torrid or Frigid zone, and offering no ground for the supposition that such a communication has existed during the recent epoch, we are led

to deny the agency of voluntary or involuntary migration in producing this dissemination. An example of this, beyond all dispute, is that of the Mediterranean Sea and Japan. No water communication for the passage of species can be imagined. An opening into the Red Sea is the only possible point of intercommunication between the two kingdoms; but this opens into the Torrid zone, in no part of which are the species found. The two regions have their peculiarities and their striking resemblances; and we are forced to attribute them to original creation and not intercommunication.

X. The resemblances found are not merely in the existence of a few identical species. There are genera common to the two seas that occur nowhere else in the Oriental kingdom, as *Latreillia*, *Ephyra*, *Sicyonia*, &c.; and species where not identical have an exceedingly close resemblance.

Now this *resemblance* in genera and species (without exact identity in the latter) is not explained by supposing a possible intercommunication. But we may reasonably account for it on the ground of a similarity in the temperature and other physical conditions of the seas; and the well-known principle of "like causes, like effects" forces itself upon the mind as fully meeting the case. Mere intercommunication could not produce the resemblance; for just this similarity of physical condition would still be necessary. And where such a similarity exists, creative power may multiply analogous species; we should almost say, *must*, for, as species are made for the circumstances in which they are to live, identical circumstances will necessarily imply identity of genera in a given class, and even of specific structure or of subgenera.

If, then, the similarity in the characters of these regions is the occasion of the identity of genera, and of the very close likeness in certain species (so close that an identity is sometimes strongly suspected where not admitted), we must conclude that there is a possibility of actual identity of species, through original creation. This, in fact, becomes the only admissible view, and the actually identical species between Japan and the Mediterranean are examples.

XI. When we find a like resemblance of genera and species between Temperate zone provinces in opposite hemispheres that are almost exact antipodes, as in the case of Great Britain and New Zealand, we have no choice of hypotheses left. We must appeal directly to creative agency for the peopling of the New Zealand seas as well as the British, and see in both, like wisdom, and a like adaptedness of life to physical nature. The Palæmon affinis of the New Zealand seas is hardly distinguishable from the common P. squilla of Europe, and is one example of this resemblance. It may not be an identity; and on this ac-

count it is a still better proof of our principle, because there is no occasion to suspect migration or any other kind of transfer. It is a creation of species in these distant provinces, which are almost identical, owing to the physical resemblances of the seas; and it shows at least, that a very close approximation to identity may be consistent with Divine Wisdom.

The resemblance of the New Zealand and British seas has been remarked upon as extending also to the occurrence in both of the genera Portunus and Cancer. It is certainly a wonderful fact that New Zealand should have a closer resemblance in its Crustacea to Great Britain, its antipode, than to any other part of the world—a resemblance running parallel, as we cannot fail to observe, with its geographical form, its insular position, and its situation among the temperate regions of the ocean. Under such circumstances, there must be many other more intimate resemblances, among which we may yet distinguish the special cause which led to the planting of peculiar British genera in this antipodal land.

The close resemblance in species and genera from Britain and New Zealand, and from Japan and the Mediterranean, and the actual identity in some species among the latter, proves therefore that, as regards the species of two distant regions, identity as well as resemblance may be attributable to independent creations, these resemblances being in direct accordance with the physical resemblances of the regions. As this conclusion cannot be avoided, we are compelled in all cases to try the hypothesis of migration by considering something beside the mere possibility of its having taken place under certain assumed conditions. The possibility of independent creations is as important a consideration. After all the means of communication between distant provinces have been devised or suggested, the principle still comes up, that it is in accordance with Divine Wisdom, to create similar and identical species in different regions, where the physical circumstances are alike; and we must determine by special and thorough investigation, whether one or the other cause was the actual origin of the distribution in each particular case. Thus it must be with reference to the wide distribution of species in the Oriental tropics, as well as in the European temperate regions, and the Temperate zone of the South Pacific and Indian Oceans.

XII. With respect to the creation of identical species in distant regions, we would again point to its direct dependence on a near identity of physical condition. Although we cannot admit that circumstances or physical forces have ever created a species (as like can only beget like, and physical force must result simply in physical force), and while we see in all nature the free act of the Divine Being, we may still believe the connexion between the calling into existence of a species and the physical circum-

stances surrounding it to be as intimate nearly as cause and effect. The Creator has, in infinite skill, adapted each species to its place, and the whole into a system of admirable harmony and perfection. In His wisdom, any difference of physical condition and kind of food at hand, is sufficient to require some modification of the intimate structure of species, and this difference is expressed in the form of the body or members, so as to produce an exactness of adaptation, which we are far from fully perceiving or comprehending with our present knowledge of the relations of species to their habitats.

When therefore we find the same species in regions of unlike physical character, as, for example, in the seas of the Canaries and Great Britain—regions physically so unlike—we have strong reason for attributing the diffusion of the species to migration. The difference between the Mediterranean and Great Britain may require the same conclusion for the species common to these seas. They are so far different, that we doubt whether species *created* independently in the two could have been identical, or even have had that resemblance that exists between varieties; for this resemblance is usually of the most trivial kind, and affects only the least essential of the parts of a species.

The continental species of Crustacea from the interior of different continents, are not in any case known to be identical; and it is well understood that the zoological provinces and districts of the land are of far more limited extent than those of the ocean. The physical differences of the former are far more striking than those of the latter. As we have observed elsewhere, the varieties of climate are greater; the elevation above the sea may vary widely; and numberless are the diversities of soil and its conditions, and the circumstances above and within it. Hence, as the creation of each species has had reference most intimately to each and all of these conditions, as well as to other prospective ends, an identity between distant continental regions is seldom to be found, and the characteristic groups of genera are very widely diverse. Comparatively few genera of Insects have as wide a range as those of Crustacea; and species with rare exceptions, have very narrow limits. Where the range of a species in this class is great, we should in general look to migration as the cause rather than original creation; but the considerations bearing on both should be attentively studied before either is admitted as the true explanation.

Throughout the warmer tropical oceans, a resemblance in the physical conditions of distant provinces is far more common and more exact than in the Temperate zone. And hence it would seem that we could not safely appeal to actual differences as an argument against the creation of a species in more than one place in the tropics. The species spread over the Oriental Torrid zone

may hence be supposed to owe their distribution to independent creations of the same species in different places, as well as to migration. Yet we may in this underrate the exactness of physical identity required in regions for independent creations of the same species. We know that for some chemical compounds, the condition of physical forces for their formation is exceedingly delicate; and much more should we infer that when the creation of a living germ was concerned, a close exactness in the conditions would be required in order that the creation should be repeated in another place. Infinite power, it is true, may create in any place; but the creation will have reference to the forces of matter, the material employed in the creation. The few species common to the Oriental and Occidental torrid seas seem to be evidence on this point. The fact that the Oriental species have so rarely been repeated in the Occidental seas, when the conditions seem to be the same, favors the view that migration has been the main source of the diffusion in the Oriental tropics.

As we descend in the order of Invertebrates, the species are less detailed in structure, with fewer specific parts and greater simplicity of functions, and they therefore admit of a wider range of physical condition; the same argument against multiplication by independent creations in regions for the most part different, does not, therefore, so strongly hold. As we pass, on the contrary, to the highest groups in Zoology, the argument receives far greater weight; and at the same time there are capabilities of migration increasing generally in direct ratio as we ascend, which are calculated to promote the diffusion of species, and remove the necessity of independent creations.

Migration cannot therefore be set aside. It is an actual fact in nature, interfering much with the simplicity which zoological life in its diffusion would otherwise present to us. Where it ends, and where independent creations have taken place, is the great problem for our study. This question has its bearings on all departments of Zoology; but in few has migration had the same extended influence as in that of Crustacea. Molluscs, if we except oceanic species, are no travellers, and keep mostly to narrow limits.

XIII. There is evidence in the exceedingly small number of Torrid zone species identical in the Atlantic and Indian Oceans, that there has been no water communication across from one to the other in the Torrid zone, during the period since existing species of Crustacea were first on the globe.

XIV. As to zoological centres of diffusion for groups of species, we can point out none. Each species of Crustacea may have had its place of origin and single centre of diffusion in many and perhaps the majority of cases. But we have no reason to say that certain regions were without life, and were peo-

pled by migration from specific centres specially selected for this end. If such centres had an existence, there is at present no means by which they may be ascertained. The particular temperature region in which a species originated may be ascertained by observing which is most favorable to its development: we should thus conclude that the *Ranina dentata*, for example, was created in the subtorrid region and not the torrid, as it attains its largest size in the latter. By pursuing this course with reference to each species, we may find some that are especially fitted for almost every different locality. Hence, we might show, as far as reason and observation can do it, that all regions have had their own special creations.

The world throughout all its epochs in past history, has been furnished with life in accordance with the times and seasons, each species being adapted to its age, its place, and its fellow species of life.*

ART. XXXIII.—*Notice of the Geological position and character of the Copper Mine at Bristol, Connecticut;* by Professors B. SILLIMAN, Jr., and J. D. WHITNEY.†

Situation.—This well known mine is situated in the north part of the town of Bristol, Connecticut, about three miles from the Canal Railroad, and four from the centre of the town, where the Station House of the Hartford and Fishkill Railroad is located.

The persevering enterprise with which this mine has been explored, the rich character of its ores, the extent and costliness of its former workings, and the general impression, lately much strengthened, that it still possesses great wealth in its ore channels, all continue to make it a subject of peculiar interest to all who feel the national value of such investments and experience. Nor is its economical value diminished by the scientific interest attaching to the peculiarly complex nature of the deposit.

Geological position and nature of the deposits.—This mine is worked in a "contact deposit," between the sandstone of the Connecticut River Valley, now supposed by geologists to be of Liasic age, and the older metamorphic rocks, commonly called primary, but in all probability of the palæozoic system.

The direction of the line of contact of the two formations at the mine is about NE and SW.

* The abstracts in this Journal from the author's Report, relating to the Geographical Distribution of Crustacea, which are here concluded, are contained in volumes xvi, xviii, xix and xx. The map illustrating the subject of Oceanic temperature, the Zones and Zoological Provinces, is in volume xvi.

† This article is taken from a "Report of an examination of the Bristol Copper Mine in Bristol, Conn., by Professors B. Silliman, Jr., and J. D. Whitney, Aug., 1855. New Haven: pp. 32. Printed for private distribution."

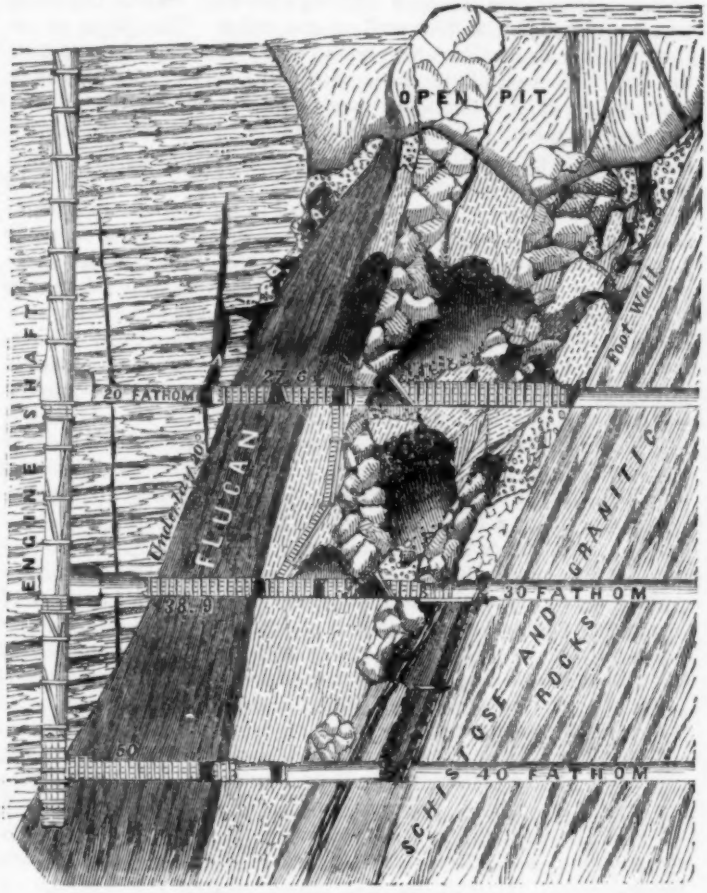
The linear extent of ground known to be metalliferous, by excavations made at various times, is between 1100 and 1200 feet. The principal workings are, however, concentrated in a space of about 600 feet north and south. There is good reason to believe that the metalliferous ground does extend at least 1500 feet, and perhaps considerably further. The mine is opened to the depth of 40 fathoms (240 feet) by an engine shaft, $6' \times 8'$ in dimensions, sunk in the sandstone, and passing out of this rock into the great ore-channel at this depth. This is at present the only working shaft of the mine, and is used for hoisting the ore, for the pit-work, and as a ladder-road to the levels. The width of the ore-ground from east to west, or at right angles to the line of contact, at the engine shaft, as exposed in the cross-cuts and levels which have been driven, is about 120 feet, and this width is maintained at the lowest depth yet reached in the mine, and the same is true for a longitudinal extent of about 500 feet, although it appears from the examination of the workings and what is known of their history, that the greatest concentration of metalliferous matter was on the surface in the vicinity of the engine shaft.

The character of the ore-ground or metalliferous belt is exceedingly complex.

Up to a recent period, however, the workings were chiefly in a series of micaceous and hornblende slates, sometimes passing into gneiss, and including large irregular "horses" of granite, which rock appears to have formed segregated masses, lying rudely parallel with the bedding of the schistose rocks. The strike and dip of these, however, is found throughout the mine to be very irregular, and there is evidence in the confused character of the ground, as well as in the slip joints and polished surfaces of the rocks, that motion of the various beds upon one another has taken place along lines of upheaval of limited extent and varying direction. The distribution of the ores in the metalliferous ground now under consideration, is found to be as irregular as is the structure of the ground itself. They consist principally of the vitreous, with some variegated ore, and a comparatively small amount of copper pyrites. The magnificent crystallizations found at the Bristol mine, especially of the vitreous ore, have given the locality a world-wide celebrity among mineralogists. In general these ores are found occurring in bunches and strings, which although preserving, usually, an approximate parallelism to the line of contact of the formations, cannot be traced continuously for any considerable distance. Hence the irregularity of the workings, especially in the upper levels, which have been extended in upon various branches of ore, or in search of others supposed to exist in certain directions, without any particular system or previously conceived plan. This has been the

TRANSVERSE SECTION OF BRISTOL COPPER MINE.

Showing the Contact of the Sandstone with the Crystalline Rocks, and the Position of the Great Flucan and Ore-ground of the Mine. Scale 60′ to 1″.



greatest drawback on the prosperity of the mine, since the ore ground was too wide to be all taken down by the miners, and the distribution of the branches of ore in it was too irregular to admit of their being found without occasional expensive excavations in dead ground. In general, throughout the mine, a tendency to a concentration of the ore around the masses of granite may be remarked, and the latter are not unfrequently well filled with strings and bunches of ore, especially near their exterior.

The limits of the ore-ground to the west, or in the direction of the older rocks, the sandstone being to the east of the contact-

line, have never been well ascertained, and must be somewhat irregular, as would be expected from the nature of the deposit. Eustis's level, going north from the 20-fathom cross-cut, has been driven along a well-marked and regular wall, which has been commonly called the foot wall of the mine. This dips, at an angle of 62° to the East, preserving a very near parallelism with the line of contact of the formations. The same kind of wall is seen in the 30-fathom level immediately beneath, where it is observed in the 20; but in neither level can it be traced for more than one or two hundred feet. There is nothing in the nature of the wall to indicate that ore might not be found beyond it, as appearances similar to this are observed in the ore-bearing ground of the mine. The excavations indicate that there was a tendency of the metalliferous matter towards this limit, and that there is not much probability of finding large bunches of ore beyond it. It is worthy of remark, however, regarding the ore-ground within the limits just described, that the average distribution of the vitreous copper is very uniform, so much so that for a long period it has been found profitable to crush and work over nearly every stone from this belt raised in working the mine.

The ore-ground just described was from the time the mine was first opened up to a recent period, considered as the only source in which the yield of copper was to be sought. Not long since, however, attention began to be called to a belt of soft rock, which lies next adjacent to the sandstone, and, of course, between it and the metalliferous belt just described. This belt is known as the "great flucan," (see diagram on previous page,) and it consists of a talco-micaceous slate completely disintegrated and softened by some chemical agency, with the exception of a few bands and nodules of hard rock, but which are of very limited extent, compared with the softer portion. The flucan contains throughout its whole mass vitreous and other ores of copper, usually disseminated through it in small particles, so as hardly to be visible to the naked eye, but occasionally concentrated into bunches and strings of considerable size. The width of the great flucan is 27 feet in the 20-fathom level, but it increases rapidly as the workings descend, being respectively 38 and over 50 feet, in the 30 and 40-fathom level. If this increase in width were to continue in depth, the flucan will meet the so-called foot-wall of the mine at or near the 80-fathom level, occupying in this place a width of about 120 feet. But that such an increase in its dimensions continues downwards indefinitely, seems hardly probable; it is more likely that it will be found to be a lenticular mass swelling out to great dimensions, and then gradually contracting again. The mass of flucan has been traced on the surface and opened at various points. At the distance of 700 feet to the southwest, from the engine-shaft, it has near the surface an inconsiderable width,

although it may increase in descending: to the north, however, it holds its width as far as it has been explored. Jilbard's level was driven in this direction from the 20-fathom cross-cut, for a distance of over 300 feet, and the flucan there found to have a width of 41 feet. Still further to the north it has been cut at the surface, beyond Ives's level, but its width in this place was found to be at 30 feet from the surface much diminished.

The deposits of ore are not entirely confined to the primary. Within the sandstone, which at the line of junction is so broken up and irregular, that its stratification can with difficulty be made out, a small seam of ore was cut in the 20-fathom cross-cut from the engine-shaft. It has been driven on both-ways from the cross-cut to a distance of about 160 feet to the north, and 120 to the south. The upper part of this course of ore is still standing, showing a narrow seam of quartz rich in ore. The level to the north is at present inaccessible: to the south the ore appears to run out at a distance of about 50 feet from the cross-cut. (This seam of ore is shown in the cross section at A.)

History of the mine.—This locality has been known for a long period as a place furnishing copper, and was opened to a very limited extent many years since. In 1836 Mr. G. W. Bartholomew made an open cut in a spot where indications of ore were abundant, and the following account of its appearance is given in 1837, by Prof. C. U. Shepard, State Geologist, who saw it at that time:

"The trench has a nearly north and south course, and extends for twenty feet somewhat obliquely across alternations of vertical layers of granite and soft mica slate. The layers of granite are from one to two feet in thickness, while those of the slate are generally much less. It was found on getting below the surface that the green malachite and brown iron ochre diminished in quantity, and that the rocks were less decomposed. Yellow copper pyrites began to make its appearance in the slate and variegated copper in the granite. The integrity of the rocks and the abundance of the ore increased regularly as the workmen descended, till at the bottom the granite presented several almost continuous veins or layers of ore which in places had a thickness of two inches."—*Report*, p. 47.

Mr. Percival says: "The Bristol Mine is chiefly opened in a large vein of a coarse reddish sub-talcose granite, adjoining which the gneissoid micaceous rock, just referred to, is also sub-talcose or sub-chloritic, with decomposed greenish chloritic seams and nodules, and with more or less copper disseminated."—*Report*, p. 77.

The property subsequently passed into the hands of Mr. A. R. Miller, who drove various shallow levels near the surface, exploring for copper. While in his possession it was visited and re-

ported on by Prof. Silliman, Senior, of Yale College, in 1839, and upon this report funds, to a small amount, were obtained in England for prosecuting the work.* But the adventurers being unable to sustain the enterprise, the explorations were subsequently continued by Mr. L. C. Ives, who erected a small steam engine, and sunk the present engine shaft in the sandstone to the 20-fathom level. During this time, and prior to 1847, considerable shipments of the ore had been made to England, but exactly what quantity we have no means of ascertaining. It appears, however, from the information obtained from the last named proprietor, that it could not have been less than 100 to 125 tons.

It was not, however, until 1846–7, that the mine was opened and worked to any considerable extent. At that time it passed into the hands of Mr. Hezekiah Bradford and his associates. Since that period a large amount of money has been expended here, and over eighteen hundred tons of copper ore have been raised and sold. In 1851 the management of the mine passed into the hands of Mr. Henry R. Sheldon, as agent for the present owners. Mr. Sheldon (who is still the manager) has had the satisfaction of seeing the mine rise from a state of great depression in its affairs and prospects to a condition of prosperity and encouraging profits, with the certainty that by a judicious and moderate outlay, it can be made to return the whole cost of its plant and extraordinary expenses, with a wide margin of profit to the adventurer.

The value of the "great flucan" in ore appears from the average of all the trials which have been made upon it for a long period, including the actual results of stamping and crushing to be over 3 per cent of ore having 30 per cent copper. In a calculation made upon the probable future product of the mine, it is assumed that the average yield will be 2½ per cent. The material composing the flucan is so soft and easily removed that no gunpowder is required in mining it. As soon as it is exposed to the action of air and moisture it *runs* as the miners express it and becomes like a micaceous clay. It weighs a little more than 150 pounds to the cubic foot. In a careful examination of the fresh material, it is not difficult to see that it is chiefly the feldspathic portion of the original material which has undergone decomposition. The outline of the feldspathic portions is plainly discovered by the friable mass of mingled silica and clay of a white color loosely filling the space occupied by the feldspar. Small garnets, of a deep red color, and small black tourmalines are constantly to be seen when the material is washed—being apparently almost the only substances beside the vitreous ore of copper which have escaped the powerful decomposing influence which has produced this great zone of altered material.

* This Report was in confirmation of one made in October, 1838, by Prof. C. U. Shepard, and which we have never seen.

In reflecting upon the probable cause of the decomposition of this extensive bed of talco-micaceous rock the suggestion naturally arises that it has probably a close connection with the injection of the trappean rocks among and beneath the sandstones of the secondary. This event it is certain was a subaqueous one, and the vast efflux of steam of high tension produced by the fiery injection of the molten trap, among rocks saturated with water must have left its traces in chemical changes produced by so powerful an agency. The irruption of the copper ores may have been and probably was an earlier event of segregation. But if the cause adduced for the decomposition of the flucan is the true one, it is pretty certain that the same decomposed material will be found at the greatest depth to which the mine will ever be explored. As it is now proposed to sink a shaft in the sandstone to the depth of 600 feet and to connect it at every 60 feet with the flucan by a cross-cut, we shall have the opportunity of studying these phenomena in detail as the explorations proceed. It is rather remarkable that the line of actual contact between the sandstone and the crystalline rocks is seldom visible and has still more rarely been carefully explored. We do not know of another example where this important feature in our geology has been explored with so much care as in the case before us.

Many statistical data embodied in our report and which are chiefly of economical value, are omitted here. It is proper to remark, however, that an important item in the present and future success of this mine is the employment of new and improved machinery, the most important of which are the ore separators and patent jigs, invented by Mr. H. Bradford. The separator is a shaking table of entirely new form and construction, on which by the rapid reciprocal motion of a copper plate the ore is by aid of water and gravity, thrown over one edge while the waste is projected over the other. After giving in detail the statistics of their operation, our report holds the following language:

Undoubtedly Bradford's separators are the most important mechanical invention ever produced for dressing what are called slime ores. The principle upon which they are constructed is truly philosophical, and has been most ingeniously applied by the talented inventor to the production of this machine. Like all accurate instruments they require careful adjustment in all particulars, but once adjusted they are completely self-acting, so that one attendant can easily oversee at least twenty of them.

Our "Report" concludes with the following observations—

In reviewing the condition of the Bristol Mine at the present time, the following are the most noteworthy points:

1. The mine is one of the few in the United States which is now worked at a profit.

2. There is a large extent of ore-ground known to exist in the mine, and which can, although of a low percentage of ores, be worked at a profit.

3. There is good stoping ground in the solid parts of the mine, which can be profitably wrought.

4. The widening of the flucan in descending, and the appearance of the solid ore-ground at the lowest points yet reached by the workings, render it a safe and advisable matter to open the mine at a considerably greater depth. Since there it is certain that the flucan may be wrought with profit, and there is good reason to believe that good leaders of ore will be cut in the solid part of the ground, and to hope that the various branches may unite into one master lode or form rich bunches of ore at the crossings of the east and west branches.

5. The position of the dressing works is such in relation to unsettled ground, that their position must be changed, or preparations made for doing so immediately.

6. A new engine shaft must be commenced to cut the lode at 80 or 100 fathoms, and must be pushed forward as rapidly as possible.

7. The ore stuff now on the surface will go far towards returning the cost of all necessary improvements requisite to place the mine in a state of great productiveness, and with proper care the old works can be kept up with undiminished returns during the erection of the new machinery.

August 26, 1855.

ART. XXXIV.—*On our Sense of the Vertical and Horizontal, and on our Perception of Distance;** by Lieut. E. B. HUNT, Corps of Engineers, U. S. A.

PHENOMENA are often unobserved or difficult of analysis, by reason of their connection with our habitual acts of consciousness. Conclusions derived from a life-long experience of our particular conditions of existence, are practically ranked as intuitions. From infancy onwards, each waking moments' perceptions go to impress these conditions upon us, as almost of absolute necessity. Our relations to space and to the earth, in their external or apparent form, are so incessantly subjects of our consciousness, that we come to regard them without special consideration or question. As a mind which has spent its habitual energies in pursuing analytical geometry, conceives coördinate axes, almost as matters of course in every investigation, so do we all, habitually experiencing the action of gravitation, come to con-

* Read before the American Association for the Advancement of Science, at Providence, August, 1855.

sider its influence, our up and down, our horizon and our general geometrical circumscription, essentially as matters of course, as intuitional perceptions, and as subjects with which reason has little concern. It is doubtless true however that we acquire such fundamental ideas from continuous experience, just as positively, as we do any other elements of our practical knowledge.

Our ideas of the vertical and horizontal and our sense of distance, have thus become so firmly established by our life-long experience, that their origin, antecedent to our philosophising exercise of reason has become much obscured. Yet they are clearly capable of rational analysis, and their component elements can be made subjects of experiment and variation. As introductory to some observations of this nature which I have made, I will present such general views as these and other experiences have suggested.

The vertical and horizontal are our habitual coördinate axes of external geometrical perception. Always ourselves, at the origin of coördinates, we refer objects seen to their respective positions, by an instantaneous perceptive location relative to these axes. We combine a mental estimate of distance with an angular reference to the vertical and horizontal axes and plane. In the horizontal plane, our perceptive usage seems to be to refer to a central line in our field of view, by angular coördinates. When we attempt to embrace the whole horizon in one mental sweep, we for the most part refer objects to the North and other compass lines; this however is a more deliberate process, and our consciousness of its steps is comparatively complete.

In the attempt to define the sources of our perception of the horizontal and vertical, two stages of causation have occurred to me as chiefly concerned in its formation.

The first of these depends on our sensibility in each part of the body and limbs, to the action of gravitation; an obscure physiological sensation of weight and pressure, in a particular direction, throughout the sensitive components of our bodily frame, a sensation which is probably greatly dependant on the circulating fluids. A specific difference of sensation notifies us of the precise inclination of each portion of our system, and could we be blind-folded and sustained in any position in the air, without any distinct consciousness of a point of suspension, we should probably still have a clear perception of the vertical at any instant. But in our ordinary position of observation, the body notifies us of the slightest variations of direction in the gravity action on each of its parts, and in the act of running down hill over rough grounds or on points of rocks, it is particularly apparent how extremely delicate is this appreciation. In this exercise our ability to combine the appreciations of ground inequalities, of our forward and downward motion of the force of gravity, and of that diminution of effective gravity due to the increasing rapidity of bodily

descent, quite suffices to make us wonder. This diminution as I have repeatedly observed, when commencing a rapid run down hill subtracts much from the force or shock of the footfalls. The skill of jugglers in balancing poles, and of rope dancers in self-balancing, are illustrations of the same gravity sensibility. When we consider the unconscious skill of the fingers of the pianist or compositor, and that in all habitual muscular acts, the tendency is to a mechanical or *quasi* involuntary action, it will not seem singular that our continuous life-long perceptions of gravitation in all our sensitive parts, should assume an obscure or latent form in our minds which would cause all its results or effects to seem intuitional. I ascribe that sense of the vertical within the limits of the body whence we pass to an acquaintance with the same in the external world, entirely to this trained gravity sensibility. In the remarkable case of John Metcalf, who though blind, was long and successfully employed in locating new roads over Derbyshire peak, this gravity sensibility must have become particularly acute, as to this only could his skill be fundamentally ascribed.

The second causal stage in our perception of the horizontal and vertical in the external world will be found to be chiefly visual. The testimony of the body to the direction of gravitation makes us aware of the angle between the vertical and the optic axes, by reason of the permanent organic relation between the body and these axes. In our ordinary or erect position, the mean of this angle or its value, when a perfect muscular equilibrium prevails, is a right angle, and any departure from this mean is indicated by the sensations of the various muscles, which cause the elevation or depression of the optic axes. Thus our sensations alone suffice to indicate to us the vertical, and a visual horizon perpendicular to it; and these sensations, checked and verified by a life-long experience, acquire an accuracy of indication which would hardly be anticipated. To these sensational coördinates, we spontaneously refer all external objects. There is very much also in our ordinary perspectives, to suggest and indicate the true horizon. Water, as the broad sheet, the winding river and the tumbling brook, level bottom lands and broad prairies, continuous hill crests, isolated knolls, either directly present horizontal planes or give such logical indications of them as the experienced eye can readily interpret, so as to derive with much accuracy a true general horizon. Indeed, we seem never to look over an extended view, without almost instantly defining with more or less precision our supposed horizon. Our previous experience from travelling over ground, doubtless enters directly in our definite location of this mental horizon. Are we in a valley, we pass our horizon under most of the objects seen. Are we in a middle point of view, we pass it in a mean position. Are we on a hill top, we pass it tangent to the general sweep of the landscape.

Thus experience is made to check and qualify the sensational indications, and we fix our horizon by a compound act of sensation and judgment from past experience.

In our mental estimate of distance, which we habitually make for all objects seen in perspective, there is doubtless a kindred combination of sensational and reflective elements. Sensation gives us a direct measure of distance for objects near at hand, while for remote objects, the element of judgment enters largely. Our normal vision being binocular, the distance between the optical centres of the two eyes, becomes a base line of constant length, to which all external distances are referred, with a precision, constantly decreasing, both outwards and inwards, from the interior limit of distinct vision. The convergence of the optic axes on each external object, is a direct result of voluntary muscular action and its particular value is indicated by a specific sensation in the eyeball muscles. Thus the third dimension of perspective is directly represented by a sensation connected with the perceiving eyes, and adjacent distances are referred, by a real triangulation, to the interocular base line. Being thus sensationally advised of the distances of objects, as far as this base continues to be an appreciable quantity relative to the perspective distances, we have, as it were, a considerable circle around us, within which we perceive, with an accuracy diminishing with distance, the absolute nearness or remoteness of objects. From the distances thus known, we pass on by an act of comparison or judgment, to those of more remote objects, where the direct sense of distance fails us. Here too, as for near objects, we doubtless receive some aid from the conscious focalization of the eye and from the gradation in the clearness of definition of outlines. All our experience in appreciating distances, gained by travelling over ground, and checking our estimates by actual comparison, also involuntarily comes to our aid. Our knowledge of the ordinary and absolute sizes of the various classes of objects seen, will besides greatly affect our rational estimates of distance. Thus sensation gives a foothold on perspective distances; comparison, or a species of continuous secondary triangulation, extends our visual scope; and experience brings in the judgment to correct false estimates.

If these views be correct, monocular vision should be much less accurate than binocular in the estimation of distances near at hand. The lack of the interocular base would make the process of perspective gaugeing almost exclusively an act of judgment. The only sensational element in the former, would be that of a conscious focalization of the eye, which could hardly be of much importance. Thus, I should conclude, that persons with only a single effective eye, would be bad judges of distance, especially among near objects. How the fact stands, I have had no means of ascertaining.

I will now instance a few observations or experiences, tending to confirm the general views here presented.

1. In passing around a particular curve of the railroad from Worcester to Boston, I have twice observed that the whole landscape on the concave side seemed to dip strongly towards the southeast, while from the adjoining views and the position relative to the ocean, I am satisfied that the real dip could not be nearly so great as the apparent. This I conceive to result from a composition in the body, of the centrifugal action with gravity, which inclined the sensible vertical from the curve centre, and so apparently lifted the sensible horizon, or depressed the actual ground relative to the same. This instance seems to show clearly the dependence of our horizon on our gravity sensibility.

2. From the summit of Crow Nest, the mountain just north of West Point on the west bank of the Hudson, the view southward presents two distinct reaches of the river in the same glimpse, which seem entirely separate bodies of water, by reason of a mountain spur, which severs all apparent connection at a distance of some six miles. From the height of over 1200 feet, you look down on the river not over a mile distant and can readily fancy this portion an elongated lake, embosomed in granite hills. Some fifteen miles below, a second apparent lake is seen, and this, despite your knowledge that it is a lower part of the same river, seems elevated above the water beneath you by perhaps 250 feet. This delusion is due, I conceive, to a dip in the apparent southern horizon, caused by our not appreciating the elevation of our point of view, relative to the hill ranges over which we look and so passing our horizon too low among the south Highland peaks. The boldness of the south face of Crow Nest hides that side from view when on the top, and so makes us think the summit of view much lower than it really is. Indeed from a point some 200 feet lower on the river face, the height above the river seems greater than that from the summit, probably because you there see the whole connecting slope. Thus in passing our visual horizon as a kind of general tangent plane, we give it a south dip, which seems to lift the distant water. This illustrates the power of our general visual habit over our gravity sensibility and our distinct knowledge.

3. From Jones' Hill, which forms the west bank of that Sleepy Hollow, where the exemplary Ichabod Crane achieved immortal fame, the west view embraces a general slope of three-fourths of a mile to the Hudson, then the broad Tappan sea, and the west bank beyond, half Palisades and half Highlands. On a quiet afternoon of last year, I there saw a thick fog so drape the west bank as wholly to hide it from view, yet leaving the east bank and about half of the river in full clear sight. The fog looked extremely sky-like and shut down in a clear line on the water.

When I simply looked sensationally on this scene, the lower fog line on the water seemed like a distant water horizon, and all above seemed but a cloudy sky. So strong was this impression, as not to be essentially impaired by seeing, half buried in the fog, the sails of a river sloop, which looked as if resting in the cloud sky. This illusion seemed to lift the hill on which I stood, to a height greatly exceeding its actual elevation. The strong likeness of the fog boundary to a sea horizon rotated the apparent horizon, so as to make it dip much below the real one, towards the west; thus overcoming both my gravity sensibility and a familiar knowledge of the locality.

4. A few weeks since, in an early morning, when standing on the bow of a sail-boat in Newport harbor, I observed, during a thick fog, a singular apparent distortion of the water surface. The boat seemed to rest in a bowl or hollow of water, some four rods in radius. This bowl seemed bounded by a gently curved swell, which ran tangent to the apparent remote horizon. Thus the water around me seemed some four or five feet lower than the horizon water. This appearance I suppose to have resulted from the fog line along the water, looking like a dim distant horizon, and thus bringing the apparent limiting horizon circle much too near. This would make the apparent horizon dip below the true, and would thus give a false apparent level to the water as far in as to that point, where in looking down on it, the true level could be distinctly seen. The true and delusive levels seemed joined by a curved swell, the curve evidently depending on the height of the eye and the density of the fog. This observation, like the preceding, illustrates the power of our idea of a water horizon.

5. If we look at the moon when a little above the horizon, its apparent enlargement may, by the simple act of shading with the hand all objects below the moon, be proved to be due in great part to the effect of intervening objects in magnifying our idea of its distance. The disc seems then to undergo a striking contraction of size. On dropping the hand, the moon seems again rapidly to enlarge. I think the effect of this masking is to give an apparent diameter not quite so small as when the moon is on the meridian, but it does not lack very much of this result. It will be noticed too, that when we see the sun or moon in the horizon, through forest branches, especially when we are travelling in a railroad car, they seem remarkably dilated. The great number of intervening branches and trunks is clearly the direct cause of this illusion.

This reasoning applies directly to explaining the apparently ellipsoidal form of the sky dome. Seeing along the horizon many objects on which the binocular or perspective sensibility fastens to elaborate the sense of distance, the horizontal axis is

much elongated in appearance, while the vertical axis, having no intervening objects to aid, is seemingly shortened. Doubtless the cause assigned by Euler, or the absorption of light by its oblique passage through the lower or misty strata in the vicinity of the horizon, may have a considerable agency in this phenomenon.

6. I was once of a party to observe Saturn through the West Point equatorial, and the several persons observing declared that the apparent size of the disc was, according to one, the size of an orange, according to another, as large as a cart-wheel, &c. In this case, the lack of perspective makes the visual angle the sole guide, except the obscure sense of focalization by which the instrumental pencils may affect the eye. Hence the utter vagueness of all estimates of apparent size, which must depend on a baseless imagination as to distance.

7. In sailing on Boston harbor, I have twice seen the phenomena of diverging and converging rays, or what is commonly called "the sun drawing water," both towards the sun in the west and towards the point symmetrically below the horizon in the east. Clear as it was rationally, that these two ray systems were but the opposite perspective views of the same parallel beams of light, I could not by any effort make them seem so sensationally. The opposite ray systems would not blend and I could not make them seem connected. The visual thinning of each beam near the perpendicular made the connection wholly invisible and so completed the illusory projection of the beams on the sky-dome. Hence an apparent widening of each beam, as it receded from the sun or from the opposite point. This whole appearance shows a complete subjection of the mental to the apparent or pictorial.

8. The great power of the two eyes to determine distances by the convergence of their axes, was strikingly shown when I was observing the reflection of a gas-burner globe from a street window of the room in which I was during the decline of day in New York. When looking with both eyes, this reflection seemed firmly established just by a branch of a tree, some ten feet outside the window. On closing one eye, I found it easy to transfer the image entirely across the street, by a mere exercise of imagination. In fact, when using only one eye, the rigid stability of the binocular vision was gone entirely, and the apparent distance seemed almost to become the subject of direct volition.

9. If in looking out over a landscape, we, by lying down or otherwise, bring the line of the two eyes into a vertical position, the scene will be found to undergo some remarkable changes. Hills will seem to recede into the distance, so as to look like far off mountains. The perspective will be found to lose very much of its relief and we seem rather to be looking at a picture on a

huge panoramic canvas, than on an actual receding perspective. Our sense of the horizon is quite impaired and a strange whimsicality of look becomes conspicuous, if we observe thus steadily for some time. In looking thus over a sheet of water, bordered by land varying from 3 to 20 miles distant, I noticed the same hollowing of the water near by, as before mentioned in the case of fog on the water. This was clearly due to the same apparent recession of near objects towards the horizon bound, and a consequent dip of the apparent horizon. It is clear too that by bringing both eyes into the same vertical, the interocular base becomes nullified for distance gaugeing in the horizontal direction. Hence objects seem to recede to one general cylinder around you, and the perspective perception is exceedingly impaired. The case is thus almost one of monocular vision. The study of these changes of scenery by our simple rotation of the interocular base, is very interesting and instructive. It shows, not only the influence of this base in forming our perspective distribution of distances, but that our sense of the horizon is very dependent for its firmness and precision on our gravity sensibility in our ordinary erect position.

Various other facts illustrative of the general views presented might be instanced, but the above must now suffice.

Art. XXXV.—*Biographical Notices of Edward Forbes and others;* by William John Hamilton, Esq.*

Gentlemen.—It now becomes my duty, in accordance with the practice uniformly adopted by my predecessors in this chair, to address to you some observations on the losses we have sustained during the past year, and it is with unfeigned sorrow that I have first to allude to one whose name can never be mentioned in these rooms without emotion. I need not say that I allude to Edward Forbes, who was endeared to us by every tie of social friendship and scientific merit, and who has been snatched away from us at the moment when he had reached the highest position his ambition could have coveted, or his admiring countrymen could have bestowed on him. Scarcely had a few short months intervened since he had been called by the universal voice of the science of Great Britain to fill the chair of Professor of Natural History in the University of Edinburgh, during which we were still regretting his departure from the metropolis, before we were astounded and overwhelmed by the unexpected announcement of his death. We felt not only individually that we had lost a valued friend, but that

* From the Anniversary Address of the President of the Geological Society of London, of Feb., 1855.

those anticipations of a brilliant scientific career, justified by the position he had attained and by the opportunities placed within his reach were doomed to bitter disappointment. These reflections are most painful, and, were I to follow my own inclinations, I would willingly forego all further allusion to the subject; but such a course would be a betrayal of duty towards our departed friend, and would disappoint the justly-founded expectations which you entertain of hearing a more detailed account of the distinguished and amiable man whose loss we so deeply deplore.

Edward Forbes was born in the Isle of Man, in the month of February, 1815. He evinced, at a very early age, an unusual taste for the study of natural history, and began to form a small museum when scarcely seven years old. A few years later he commenced his geological studies with the perusal of Buckland's 'Reliquiæ Diluvianæ,' Parkinson's 'Organic Remains,' and Conybeare's 'Geology of England,' exhibiting at the same time a more than usual taste for drawing.

He visited London at the age of sixteen, and was then engaged in studying drawing under Sass, but this was not enough to occupy his eager and ardent mind. He proceeded in 1831 to Edinburgh, where he devoted his whole time and energies to the pursuit of his favorite subject of natural history, while professing to overcome his repugnance for the study of medicine, the ostensible object of his matriculation. But medicine as a profession had no charms for one whose whole soul was filled with a love of the beautiful, and with an intense admiration of the works of Nature in every varied form. He cultivated his taste for natural history under the able teaching of such men as Professors Jameson and Graham. He delighted particularly in the botanical excursions of the latter, who was accustomed periodically to lead forth his pupils to the Highlands; thus making Nature herself, in her truest and loveliest garb, afford the practical illustrations of the teaching of the class-room.

At this period of his life, scarcely a year passed without some botanizing or dredging excursion, and long before he arrived at manhood, he had made himself well acquainted with the Fauna of the Irish Sea, on the shores of his native island. At the age of eighteen, in company with a fellow student, he made an excursion to Norway, where he spent some weeks exploring the wild and romantic districts of the country, adding to his zoological and botanical observations. Already at this time Edward Forbes began to direct his attention to botanical geography, the forerunner of those deep and philosophical views respecting the geographical distribution of the Flora and Fauna of the world which he subsequently developed, and which constitute one of the most interesting and leading features of all his writings.

In 1835, Edward Forbes visited the Alps; in 1837 he was prosecuting his studies at Paris under Beudant, Prévost, Geoffroy St. Hilaire, and De Blainville, and in May of the same year we hear of him at Algiers; the result of this expedition was an account of the land and freshwater mollusca of Algiers and Bougia, published in the 'Annals of Natural History' for May, 1839.

With the same view of prosecuting his researches in natural history, he visited Styria and Carniola in 1838, his remarks on which were published in the 'Proceedings of the Botanical Society.' In the summers of 1839 and 1840 he delivered at Edinburgh, whilst still a student, a course of scientific lectures on zoology, as well as one of a more popular nature, in which he pointed out the bearings of zoology on geology. I mention this as a subject of peculiar interest to us, as indicating the commencement of those views which, by their subsequent development and their growing importance in the hands of Edward Forbes, have exercised such a beneficial and practical influence on the study of geology.

The time was now fast approaching when Edward Forbes was to find a wider sphere for the exercise of his brilliant genius. In 1841 he published his 'History of British Star Fishes and other Echinoderms,' a delightful volume, charmingly illustrated by his own pencil and from his own designs. There are many in this room who will recognize in these illustrations the same ingenious and playful fancy, and the same ready pencil which never allowed a sheet of paper to lie unused before him, while he had a chance of transferring to it the humorous and graceful forms which he realized without an effort, and almost without a thought. In this same year he obtained the appointment of naturalist to H. M. surveying ship *Beacon*, Captain Graves, then employed in completing the survey of the coast of Asia Minor and the adjacent islands: an appointment more suited to his tastes and to his talents could not have been devised. He had here full play for the prosecution of his favorite pursuits of botany, zoology, and geology. Already well acquainted with the flora and fauna of the European Continent and their geographical distribution, he had now an opportunity of tracing their further extension to the East, and of examining the first appearance of the Oriental *facies* which they put on in the eastern portions of the Mediterranean. Nor was Edward Forbes the man to neglect such an opportunity. During this and the following year he pursued his botanical and zoological researches with unwearied energy, assisted by Captain Graves, who omitted no opportunity of enabling his scientific friend and companion to avail himself of every occasion for observation which the service afforded. It was during his various excursions in the *Beacon* and her boats, that Edward Forbes followed out those researches with the dredge, amongst the islands of the

Ægean Sea and on the adjacent coast of Asia Minor, which alone would have immortalized his name. At the same time he neglected no occasion of studying the geology and botany of the regions which he visited; but the dredge and its results will ever remain the chief glory of this expedition. The results of these researches were made known to the public in the 'Report on the Mollusca and Radiata of the Ægean Sea, and on their distribution, considered as bearing on Geology,' made to the British Association at their meeting at Cork in 1843. From this report it appears that the data on which it was founded were entirely derived from personal researches during a voyage of eighteen months in the Ægean, when but a few days passed by without being devoted to natural history observations. The calculations were based on more than 100 fully-recorded dredging operations in various depths from 1 to 130 fathoms, and in many localities from the shores of the Morea to those of Asia Minor. And with that modesty which ever characterized Edward Forbes in all his works, he adds, that the merit of the results is mainly due to Captain Graves. The chief objects of the report, as stated by the author, were, "to give an account of the distribution of the several tribes of mollusca and radiata in the Eastern Mediterranean, exhibiting their range in depth and the circumstances under which they are found; to inquire into the laws which appear to regulate their distribution, and to show the bearings of the investigation on the science of geology."

I shall not attempt to give an analysis of this valuable report; I shall content myself with reminding you of some of the more important conclusions, as bearing on geological investigations, which are embodied in it. The most important fact which has resulted from them respecting the development and distribution of animal and vegetable life in the depths of the ocean is, that of the almost uniform occurrence of particular species in particular zones of depth below the surface. This distribution of marine animal life is determined by three primary, modified by several secondary, influences. The primary are climate, sea composition, and depth; of the many secondary influences, the most important is the character of the sea bottom. According as rock, mud, sand, weedy or gravelly ground, prevails, so will the number of the several genera and species vary. The outline and geological nature of the coast is also an important feature in modifying the marine fauna. Other secondary influences are tides and currents, the influx of fresh water, &c.

We have then a full description of eight well-marked regions of depth in the Eastern Mediterranean, each characterized by its peculiar fauna, and when there are plants by its flora. These regions are distinguished from each other by the association of the species they severally include. Certain species in each are found

in no other, several are found in one region which do not range into the next above whilst they extend to that below, or *vice versâ*; a certain species have their maximum of development in each zone, being most prolific in individuals in that zone in which is their maximum, and of which they may be regarded as especially characteristic. Every zone has also a more or less general mineral character, the sea bottom not being equally variable in each, and becoming more and more uniform as we descend. Again, the deeper zones are greater in extent, so that whilst the first or most superficial is but 12, the eighth, or lowest, is above 700 feet in perpendicular range; its horizontal extent increases in a somewhat similar proportion. Another significant feature is, that as we reach the eighth zone, the number of species and of individuals diminishes as we descend, pointing to a zero in the distribution of animal life as yet unvisited. Species disappear in depth which do not seem to be replaced by others. From other observations the following general inference is deduced; that the extent of the range of a species in depth is correspondent with its geographical distribution.

But these eight regions are themselves the scene of incessant change; not only are the depths modified by the addition of fresh matter, but the very animals themselves, by their own increase, so modify the nature of the sea bottom as to render it unfit for their own existence, until a new layer of sedimentary matter, uncharged with living organic contents, has formed a fresh soil for similar or other animals to thrive on. It is impossible to overlook the importance of these observations in explaining many of the daily recurring phenomena which are brought under the notice of the geologist; in the last observation we may see an explanation of the phenomenon of interstratification of fossiliferous and non-fossiliferous beds.

I must refer you to the report itself for an account of the phenomena which would be presented to us were the bottom of the Ægean Sea to be elevated and converted into dry land, or to be filled up by a long series of sedimentary depositions. He concludes by observing that, "supposing such an elevation to have taken place, a knowledge of the association of species in the regions of depth would enable us to form a pretty accurate notion of the depth of water in which each bed was deposited. A beautiful illustration of this argument is given from observations made on the island of Santorin, and under different circumstances the contrary observations might be made; the geologist is thus enabled, by a careful examination of the successive overlying groups of species, to ascertain whether in any given locality brought under his notice, the sea bottom was being elevated or depressed."

But I have already dwelt too long on this report; I must hasten to other scenes in the life of Edward Forbes. During his

stay in the Mediterranean, he made several excursions into Lycia, where he had an opportunity of combining his love of art with the pursuit of natural history. On one occasion, in company with Mr. Hoskin, they discovered and fixed the sites of two of the Cibyratic cities. A second excursion undertaken with the Rev. Mr. Daniell and Captain, then Lieutenant, Spratt, was still more important; the sites of eighteen ancient cities hitherto unknown to geographers were explored and determined, and the names of fifteen were identified by inscriptions found amongst the ruins. During this expedition Mr. Daniell fell a victim to the malignant malaria of the country, and the life of Edward Forbes himself was at one time in danger. Indeed there can be little doubt that at this time were sown the seeds of that disease which has eventually deprived us of his services. He, however, gradually recovered, and was on the point of proceeding to Egypt and the Red Sea on a dredging excursion, when he was informed that he had been elected to fill the Chair of Botany in King's College, vacant by the death of Professor Don. He returned immediately to England, and, on the 8th May, 1843, delivered his inaugural lecture in that institution.

But previously to this event, Professor Forbes had become intimately connected with this Society. At the close of 1842, Mr. Lonsdale, who for so many years had been the curator of our museum, resigned his office in consequence of the state of his health. In the report of the Council read at the Annual General Meeting on February 17, 1843, I find the following passage, after alluding to the loss sustained by the resignation of Mr. Lonsdale: —"In recording the election of his successor, the Council cannot omit to congratulate the Society on having secured the services of such a distinguished naturalist as Mr. E. Forbes." I may appeal to the recollection of every member of the Society for a confirmation of my statement, that the expectations then entertained, great as they unquestionably were, were more than fulfilled by the manner in which Edward Forbes conducted the business entrusted to him during the period that he held this important office. The report of the Museum Committee for 1844 will show how his labors were appreciated by the Council. But before the close of the same year his talents as a naturalist and a palæontologist called him to a more extended sphere of action. On the establishment of the Museum of Practical Geology in connection with the Ordance Geological Survey under the direction of Sir H. De la Beche, Professor Forbes was appointed palæontologist to that institution, and resigned the curatorship of the museum of this Society. On the removal of the Museum to Jermyn Street he was appointed its Professor of Natural History.

Here then his talents had full space for their development, and Edward Forbes was not slow in bringing to bear on his numerous

avocations the knowledge he had so industriously collected. Combining as he did a lively and vivid imagination with a mature and well-disciplined judgment, he was enabled to employ with effect that power of generalization and abstraction which he so eminently possessed. His enlightened and comprehensive views on the numerous branches of natural history which he cultivated, and which were founded mainly on his own experience, caused him from henceforth to be looked up to as one of the first of British naturalists, and the works which he now published bear ample testimony to his well-founded reputation. Nor was it in England alone that his merits were recognized. In France, in Germany, in Italy, wherever men of science were to be found, the name of Edward Forbes was equally acknowledged as deserving a place in the first rank of scientific merit.

Towards the end of 1846, he published with Lieut., now Captain Spratt, an account of his travels in Lycia, a work in which we are at a loss to know whether most to admire the admirable details of archæology and art, or the equally graphic description of the botany, geology, and zoology which it contains. About this time appeared in the Proceedings and Transactions of our Society, his monograph on the South Indian Fossils sent to this country by MM. Kaye and Cunliffe and the Rev. W. H. Egerton. The report itself independently of the description of the fossils, is short, but it is not the less important, and is eminently characteristic of the author. He points out the general resemblance of the *facies* of the fossils to that of the Cretaceous period of Europe, and more particularly the lower portions of that series. His arguments are drawn rather from similarity, than from identity of species; a subject to which he had particularly directed his attention during his researches in the Ægean Sea. The report is preëminently suggestive, and I would particularly mention that portion of it which refers to the occurrence in these Cretaceous beds of certain forms which are usually considered as characteristic of Tertiary formations, and which very forms are now found in their greatest assemblages living in those eastern seas,—a fact, which, he observes, goes far to support the theory, that genera, like species, have geographical birthplaces as well as geographical capitals.

About this time, also, he wrote one of the most remarkable contributions to the science of Geology, which has appeared in this country. It is published in the first volume of the Memoirs of the Geological Survey of Great Britain, and is entitled "On the Connexion between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological changes which have affected their area." "In this work," to use words already printed, "the happy combination of great botanical and zoological knowledge is made to bear on some of the most intricate inquiries with regard to the age and relationship of the rocks of Great Britain."

Mr. Horner, when President of this Society, has borne his ready testimony to the merits of this work, when he says in his Anniversary Address in 1847, that this Essay "is an admirable example of the light to be derived from other branches of natural history in the prosecution of geological inquiries; of the application of animal and vegetable physiology, and a knowledge of the habits and distribution of animals and plants to the elucidation of very difficult problems in geology." Mr. Horner, in the Address from which I have quoted these words, has given an admirable account of this interesting and attractive memoir, so suggestive as it is of great and enlightened views. I will therefore here only observe, that the principal theory which it is the object of this Essay to establish, is based on the assumption of the existence of *specific centres*, that is, of certain geographical points from which the individuals of each species have been diffused, involving their consequent descent from a single progenitor, or from two, according as the sexes might be united or distinct. Prof. Forbes further declares, as his opinion, that the "abandonment of this doctrine would place in a very dubious position all evidence the palæontologist could offer to the geologist, towards the comparison and identification of strata, and the determination of the epoch of their formation." Having assumed the truth of the doctrine of specific centres, the problem which he proposes to solve is the origin of the assemblages of the animals and plants now inhabiting the British Islands. Within this limited area he considers that the united labors of British naturalists have shown that there are a great number of animals and plants which are not universally dispersed, but are congregated in such a way as to form distinct regions or provinces. The vegetation, for instance, presents five well-marked Floras, four of which are restricted to definite provinces, whilst the fifth, besides exclusively claiming a part of the area, overspreads and commingles with all the others.

Prof. Forbes considers that, of the three given modes by which an isolated area may become peopled by animals and plants, "immigration before isolation" of the area was the mode by which the British Isles have chiefly acquired their existing flora and fauna, terrestrial as well as marine, and that it took place subsequently to the Miocene epoch. It follows from this argument, that previous to the isolation of this area, it must have been in direct union with those portions of the European continent the floras of which are shown to be identical with one or other of the five floras of the British Isles. I will briefly mention the five distinct floras which he has noticed, and the districts with which he considers they prove our former connexion.

1. *The West Irish Flora.*—The high lands in the North of Spain present the nearest point where a vegetation occurs identical with that which is characteristic of the mountainous district

of the west and southwest of Ireland. Consequently, at some period or other, continuous dry land must have existed from the coast of Spain to that of Ireland.

2. *The Devon Flora*, connected with that of the Channel Islands and the neighboring parts of France.

3. *The Kentish Flora.*—The vegetation of the southeast of England is distinguished by the presence of a number of species common to this district and the opposite coast of France.

4. *The Alpine Flora.*—On the tops of some of our most lofty mountains, particularly in Scotland, are plants not found elsewhere in the British Islands, but which are identical with those of the Scandinavian Alps, thus pointing to a former connection in that direction.

5. *The General Flora.*—This universal flora is almost identical as to species with the flora of central and western Europe, and may be properly styled Germanic.

The arguments by which these views are maintained are clearly and satisfactorily developed, but must be read and studied to be appreciated. That portion of the paper, however, which relates to the distribution of the marine plants and animals now inhabiting the British seas is still more deserving of careful study. The account of the distribution of the British Mollusca is particularly so; it contains a mass of information on the subject, not to be found, at the time of its publication, in any one work, and of the greatest value to the student of Tertiary geology. I will only mention one or two of the more interesting points with which the memoir concludes.

"That the flora and fauna, terrestrial and marine, of the British Islands and seas have originated, so far as that area is concerned, since the Miocene epoch.

"The greater part of the terrestrial animals and flowering plants now inhabiting the British Islands are members of specific centres beyond their area, and have migrated to it over continuous land, before, during, or after the glacial epoch.

"All the changes before, during, or after the glacial epoch appear to have been gradual and not sudden, so that no marked line of demarcation can be drawn between the creatures inhabiting the same element and the same locality during two proximate periods."

Of the many scientific papers of great merit which Prof. Forbes subsequently published, in our own and other Journals, I will only allude to one, which in this room cannot be passed over in silence. In his paper 'On the Fluvio-marine Tertiaries of the Isle of Wight,' published in the 9th vol. of our Quarterly Journal, the result of the laborious investigations of several months, he has established, on data which cannot be questioned, the true order of superposition of the upper tertiary beds of that typical locality,

correcting the errors of previous inquirers, and confirming a suggestion made by Mr. Prestwich, that the strata composing a part of Hempstead Hill were probably higher than any beds hitherto noticed. The result of Prof. Forbes's inquiries has been to show that, taking the Whitecliff Bay section for an example, the Headon Hill beds, instead of constituting the highest portion of the series, are overlaid by several other distinct formations, consisting of the St. Helen's or Osborne beds and the Bembridge series, the latter consisting of several distinct divisions, all characterized by peculiar fossils, chiefly, however, freshwater or brackish. He has, moreover, distinctly ascertained that the Hempstead Hill series constitutes another subdivision overlying the uppermost bed of the Bembridge series, and characterized by a fresh set of fossils. "Thus," to use the author's words, "we find that the fluvio-marine Eocenes of the Isle of Wight are more than twice as thick as they have hitherto been regarded, and that the additional beds are even of greater geological importance than those hitherto recognized."

The remarkable feature in this section is, that from the Barton series upwards there is no break in the series of deposits; and as Prof. Forbes identifies the Hempstead series with the middle, and possibly the upper Limburg beds of Belgium, he is logically led to the conclusion, that the Limburg beds, and consequently the Weinheim beds of the Mayence basin, which are unquestionably of the same age as the Middle Limburg, must be also Eocene. Other continental beds are also referred to as necessarily belonging to this Eocene period. This is not the place to offer any criticism on Edward Forbes's conclusions, but I may perhaps hereafter allude to the question, for the purpose of testing whether there may not exist some flaw in the argument, by which so many of the younger continental beds are drawn into this Eocene vortex.

During this period Prof. Forbes was not only most industrious with his pen, but he was unwearied in his arrangement and classification of the vast accumulation of fossils collected by the Ordnance Geological Survey, and now exhibited in the Jermyn Street Museum. He was no less active in the field with his hammer and his note-book. He not only explored various parts of England, Wales, and Ireland, but he visited with the same observant eye and comprehensive glance, many portions of Belgium and of France, carefully comparing their various aspects and phenomena, and procuring materials for his philosophical generalizations.

It is unnecessary for me to remind you of the satisfaction with which we hailed his appointment to the Presidentship of this Society, looking forward to the influence of his profound knowledge of palæontology on the future progress of our science. But scarcely had he occupied his chair for half the allotted term, when

the death of his old master, Prof. Jameson, was announced in this metropolis. The universal voice of science was not slow in recognizing Edward Forbes as the man who, above all others, both as a naturalist and a geologist, was most fitted to succeed him. At the same time, I am bound to say, that while we were all ready to congratulate him on the prospect of thus reaching the highest goal which a true naturalist could desire, we looked forward with regret to the prospect of his removal from our circle. Nor was this grief altogether free from a feeling of shame, that this vast city, with its wealth, its display, its riches, its public and private associations, its great collections, its lavish expenditure, and in many respects its unbounded liberality, could propose no prize, no reward to the scientific man worthy to be placed in competition with that offered by the northern capital. Little did we then imagine that the regret we felt at his departure from amongst us was destined to be so soon merged in another, as overwhelming as it was altogether unexpected. Little did we imagine that the fond anticipations of a long and glorious career for our friend, in which we then indulged, were doomed to be so speedily destroyed.

Prof. Forbes was appointed to the vacant Chair of Natural History in the University of Edinburgh. He had thus obtained the great object of his life. An intimate friend, writing in one of the Edinburgh journals, says that he considered all his plans, excursions, observations, &c., as preparatory to this one object. During his active and laborious life, all his hopes and future plans pointed to Edinburgh as the only appropriate place for developing that vast amount of natural history acquirement he had obtained. There he looked forward, amongst other things which his eager fancy had prepared for him, to the formation of a magnificent museum, arranged according to that system which for years he had been zealously maturing. Nor can there be any doubt, but that with the liberal support of government, assisted by that of private individuals, he would have been enabled in a few years to carry out his plans. But alas! scarcely had he reached that goal which he had spent his whole life in endeavoring to attain, and which he was anxiously preparing to adorn with all the ornaments of science collected from every quarter of the globe, when he was suddenly carried off by the inscrutable decrees of Providence; and the glorious fabric he had erected,—that mental storehouse filled with the treasures of many years' collecting, fell to pieces before our eyes, and nothing remained but the broken fragments and the shattered scaffolding, to be again dispersed and scattered, without system and without order, until they should be again hereafter collected together with infinite labor and fatigue by some future master-mind.

The fate of Sir JOHN FRANKLIN has long been a mystery to his countrymen: he has probably long ceased to be a member of this Society. It is, however, only during the course of the past session that any authentic information has reached this country that the gallant explorer of the Arctic regions, with his adventurous followers, had ceased to exist. Far from their ships, which, in the extremity of danger and a hard struggle for life, they must have abandoned, and after vainly endeavoring to reach a more southern and hospitable region through a trackless desert, their remains were discovered by travelling Esquimaux, from some of whom portions of their property were obtained. These were rescued by the intrepid Dr. Rae, who had gone in search of them overland, and who brought back the melancholy certitude of their fate. Their bones now lie whitening on the Arctic shore, or beneath fields of eternal snow. By what means they reached that spot, or how they perished, will probably never be known; but their memories will ever be cherished as of men who risked and sacrificed their lives in the performance of duty and of scientific inquiry, and I trust I may also add, as the last installment of valuable lives sacrificed to a vain and chimerical attempt to discover that which, could it ever be discovered, would be alike unprofitable and unavailable.

Rear-Admiral Sir John Franklin was born at Spilsby in the year 1786, and performed his earliest service in the navy in the first year of this century, as a midshipman on board of the Polyphemus at the battle of Copenhagen. Sailing afterwards with Capt. Flinders to Australia, he acquired that skill in surveying and that power of observation which characterized his subsequent career. After serving in the engagement against Admiral Linois in the Straits of Malacca, he next acted as signal-midshipman of the Bellerophon in the glorious victory of Trafalgar; and, lastly, towards the conclusion of the great war, his gallantry was again displayed conspicuously in the naval attack upon New Orleans, for which conduct he obtained his lieutenancy.

A peace being established which promised a long duration, Franklin sought to be employed in the most adventurous service in which a seaman could then be engaged. He obtained, through the patronage of Sir Joseph Banks, the command of the surveying vessel, the Trent, being one of two ships under the orders of Capt. Buchan, destined to penetrate into the Polar Seas; on that occasion Franklin not only reached the high latitude of 84° 34″ N. lat. in the meridian of Spitzbergen, but evinced a strong desire to be allowed to proceed onwards alone, in the endeavor to effect a through passage.

The undaunted and inflexible perseverance which he exhibited in his explorations off the coast of North America, between the years 1819 and 1822, both inclusive, is well known to the public

through the clear and emphatic productions of his own pen. As geologists, however, we must specially remember, that the rock-specimens then brought home by Franklin, and his associate, the eminent naturalist, Richardson, first revealed to us the structure of those distant and inaccessible regions.

On his return to England, however, Franklin felt so strongly the want of better geological knowledge on his own part and on that of his officers, that when appointed to the command of the next Arctic expedition, on which he sailed in 1825, he took his first lessons in our science at the museum of our Society, accompanied by his distinguished companions, Back and Richardson. At these morning meetings our much-respected former President, Dr. Fitton, was the instructor, assisted by Mr. T. Webster, then our Secretary; Sir Roderick Murchison, who has informed me of these circumstances, being then also one of the learners.

The intimacy thus commenced continued till Franklin's last departure from the shores of Britain in 1845; for whether he was treading unknown tracts of North America, or commanding the Rainbow frigate in the Mediterranean, or performing the duties of Governor of Van Diemen's Land, our deceased member, having been knighted by his sovereign and duly honored by various public bodies, never ceased to correspond with his scientific friends, including Mr. Robert Brown, the enlightened botanist, and the old officers of this Society, Fitton and Murchison; sending to them also many specimens or descriptions which might, he thought, advance human knowledge.

As Sir John Franklin united the warmest heart and kindest manners to a solid understanding, it naturally followed that his friends took an intense interest in promoting all those endeavors to rescue him and his followers from their last perilous voyage, and in encouraging every effort directed to that end, whether made by the government or by the magnanimous Lady of the missing chief. The successive Presidents of the Royal Geographical Society, and particularly Sir R. Murchison, stimulated our rulers to make every possible research which might lead to the timely discovery of the absent voyagers. How some one of the earliest of these efforts might have succeeded, had it taken a southerly direction from Barrow's Straits, is indeed now established by the melancholy announcement made by Dr. Rae; for, although the party was supplied with provisions for three years only, we now know that a large remnant of the force had certainly sustained life for five years.

The late Professor JAMESON was the third son of Thomas Jameson, Esq., and was born at Leith on the 11th July, 1774. In his early years he showed a strong desire to become acquainted with natural objects, the study of which he evidently preferred to that of books and letters. His first attempts were made in stuff-

ing birds, and in collecting animals and plants on the beach of Leith and its vicinity. A strong desire to travel was the result of his favorite pursuits, and his father ultimately yielded to his often-repeated wish to enter on the profession of a mariner; but his friends interposed, and suggested that by adopting the study of medicine, he might equally be enabled to study the works of nature. He yielded in his turn, and was appointed assistant to the late John Cheque, Esq., surgeon in Leith. He commenced his study of natural history in 1792, under Dr. Walker, then Professor of Natural History in the College of Edinburgh, and soon became a favorite pupil. In 1793 he visited London, and became acquainted with the principal scientific men of the metropolis, and ever after spoke of the pleasure and benefit he had derived from his intercourse with Sir Joseph Banks, Mr. Dryander, Dr. Shaw, and other leading members of the Linnæan Society. With the exception of comparative anatomy, he now abandoned all idea of pursuing his medical studies. His attention was directed to the sciences of ornithology and entomology, then of chemistry, and subsequently of mineralogy and geology, including a thorough knowledge of analytical chemistry. In 1797 Prof. Jameson paid his first visit to the island of Arran, and in the following year he published his work on the 'Mineralogy of the Island of Arran and the Shetland Islands, with Dissertations on Peat and Kelp.' It was the first good geological account of these places and formations, and soon acquired a well-merited celebrity. He subsequently visited other portions of Scotland, and in 1800 published his 'Mineralogy of the Scottish Isles,' in two vols. 4to, illustrated with maps and plates. This work contained the first sketch of the geology of the Hebrides and Orkneys.

But the real period of Jameson's celebrity as a mineralogist and a geologist dates from the year 1800, when he left his native country for Freiberg, where he remained nearly two years studying mineralogy and geology under the famous Werner. Jameson fully acknowledged that it was from him he first derived clear and distinct views of the structure and classification of rocks. This opinion is confirmed by Conybeare, who says, "We are chiefly indebted to the reports of Werner's pupils, especially to those of Jameson, for our knowledge of Werner's general views, so fully developed in his lectures, and there only." Jameson also observed, in a passage which is too important not to be quoted on this occasion, pointing as it does to the very fundamental principle of all our modern geological investigations, that "Werner taught that mineralogical and geological characters, and characters derived from organic remains, were to be employed in determining formations, and that probably the same general geological arrangements would be found to prevail throughout the earth. But," he added, "the truth or falsity of this view in regard to the

similarity of formations, can only be determined by the united labors of geologists continued for a long series of years." This, it may be observed, is the very position our science now occupies, tracing out geological formations from one hemisphere to the other, referring the fossils of India to the age of the Chalk of England, and comparing the Palæozoic fossils of Australia with those of Great Britain and America. Thus, as Prof. Jameson admitted, it is to Werner that we are principally indebted for our present highly interesting views of the natural history of fossil organic remains; and in confirmation of this opinion, Prof. Jameson at a subsequent period vindicated the geognosy of Werner from the attacks made upon it by the Edinburgh Review.

In 1804 Jameson returned to England in consequence of the state of his father's health. Shortly afterwards, on the death of Dr. Walker in the same year, Jameson was appointed Professor of Natural History; and from that period, by his admirable lectures, founded in a great measure on the sound mineralogical and geological views of his friend and master the Professor of Freiberg, he raised the Edinburgh school of Natural History to the proud preëminence it has occupied for the last half-century. In the same year, he published the first part of the first volume of his 'Mineralogical Description of Scotland;' his other labors, however, prevented the completion of the work. In 1808 he founded at Edinburgh the Wernerian Natural History Society, of which he was elected perpetual President.

In 1809 he published the 'Elements of Geognosy,' a work which contributed more to introduce the doctrines of the Wernerian school into England than any other publication; and from this time may be dated the antagonism between the Wernerian and the Huttonian doctrines, as advocated by the northern geologists. Nor was the spirit of partisanship thus engendered altogether useless, inasmuch as its final effect was to call attention to the study of, and to diffuse a more general taste for, geology. Independently of this, the modification of the Neptunian theory as adopted by Werner, and in which form Prof. Jameson introduced it to the notice of his countrymen, has been proved by the test of modern science to be more consistent with the phenomena of Nature than the Plutonian views of its adversaries. It has served to introduce a more methodical study of the different formations of the earth's crust, in harmony with the numerous organic remains which they contain, and which never could have been reconciled with the doctrines of the Huttonian theory.

In 1813, at the suggestion of Professor Jameson, a translation of Leopold von Buch's 'Travels through Norway and Lapland in 1806, 1807, and 1808,' was published by Mr. Black,—Jameson himself adding to the interest of the work by an account of the author, and by various notes illustrative of the natural history of

Norway. In 1816, another edition of the 'System of Mineralogy' made its appearance in three volumes; and at the same time a new edition of his 'Characters of Minerals' was called for. Other editions of both works followed. In 1819, he commenced the 'Edinburgh Philosophical Journal.' For the first six years he conducted it with Sir David Brewster, but after that period he was the sole editor. It extends to seventy volumes, and is one of the most valuable repositories of scientific information in Britain. It will ever form one of the most durable monuments of his talents and industry.

But while Jameson was thus exerting himself in Edinburgh to propagate sound and correct views respecting the geological phenomena of the earth's crust, another distinguished naturalist was laboring in another capital to bring about the same results by the help of comparative anatomy.

In 1821, the immortal Cuvier published his 'Discourse on the Theory of the Earth,' as an introduction to his 'Researches on Fossil Bones.' To Professor Jameson we are indebted for the publication of a translation of this work made by Mr. Kerr. On this work Jameson observes:—"The notes I have added will, I trust, be found interesting, and the account of Cuvier's 'Geological Discoveries' which accompanies them will be useful to those who have not an opportunity of consulting the great work." This popular work produced an excellent effect in this country, for Cuvier was but partially known in England until this essay appeared. It rapidly ran through five editions: in the fifth, Professor Jameson entirely remodelled it, extending it from 190 to 550 pages.

During this period he also contributed many articles to the 'Encyclopædia Britannica' and to the 'Edinburgh Encyclopædia;' and on the return of Captain Parry from his Polar Expedition he drew up, from the specimens brought home, a sketch of the geology of the different coasts discovered and touched at by that enterprising navigator. But it would be occupying too much of your time, to enumerate the various works which flowed from his ever-ready pen. I cannot, however, conclude this notice without briefly alluding to one point respecting which Prosessor Jameson deserves the greatest praise, both for what he effected and for what he endeavored to effect. The present Museum of Natural History in Edinburgh is the result of Jameson's unceasing industry and efforts. The collections which existed before his time were almost entirely removed by the Trustees of his predecessor, Dr. Walker; and the nucleus of the present magnificent collection was Professor Jameson's private property, when he was called to fill the chair of Natural History. He labored incessantly to render it worthy of the place; but the means placed at his disposal, both by the Town Council and the goverument, were inadequate

to the task, and it was not without great private outlay that Professor Jameson raised it to its present state. In fact it may be said that the present Museum was founded, created, arranged, and exposed for public exhibition by the head and the industrious hands of Jameson alone. Professor Jameson died in Edinburgh, at the age of eighty, on the 19th of April, 1854.

The name of Arthur Aikin is associated with the earliest days of the existence of our Society. In that Charter which forms the basis of our constitution, his name occurs as one of the founders of this Society. He was born at Warrington, in Lancashire, on the 19th May, 1773. The grandson of John Aikin, D.D., eminent for his learning and abilities, he evinced at an early age a decided love for literature and science, and from his father derived a taste for zoology, for chemistry, and for English botany. An early acquaintance with Dr. Priestly, of whom he subsequently became a favorite pupil, and whom he assisted in the arrangement of a new laboratory, confirmed him in his predilection for chemistry. In 1797 he published an account of a tour in North Wales, made in the previous year in company with his brother Charles and another friend, under the title of 'Journal of a Tour in North Wales and part of Shropshire, with observations in Mineralogy and other branches of Natural History.' At a subsequent period, in conjunction with his brother, he delivered lectures on Chemistry and Chemical Manufactures, of which a syllabus appeared in 1799. In 1807 he published 'A Dictionary of Chemistry and Mineralogy,' 2 vols. 4to; and in 1814, 'An account of the most recent discoveries in Chemistry and Mineralogy.'

But before this time Arthur Aikin had become conspicuous as one of that distinguished band of scientific men who contributed to the formation of the Geological Society of London, and founded it in 1807; soon afterwards his knowledge of mineralogy and chemistry must have contributed to his being appointed one of the Secretaries of the Society. In the first volume of the first series of our Transactions, published in 1811, his name appears as one of the Members of Council. In the second volume, published in 1814, he appears as one of the Secretaries, as well as in the third volume, published in 1816; but there is reason to believe that he became one of the Secretaries at a still earlier period. In the first volume of the first series there is an interesting paper by him, entitled 'Observations on the Wrekin and on the Great Coal Field of Shropshire;' and in the third volume is another with the title of 'Some Observations on a Bed of Trap occurring in the Colliery of Birch Hill, near Walsall, in Staffordshire.' These papers, like all those published by the Society at that period, were of a much more mineralogical character than those now constituting the bulk of our publications. Palæontology had then made but little progress. Its value and importance in assisting our

knowledge of the relative ages of rocks was hardly recognized, nor amongst the illustrations which accompany the early volumes are there any figures of organic remains. At a subsequent period he was appointed to the Secretaryship of the Society of Arts; this circumstance is supposed to have led to his retirement from the office of Secretary to this Society; but he continued for many years longer to serve on the Council, of which he was a member for the last time in 1830. One of the earliest members of the Society who knew him well thus writes to me of him:—"He had a very logical head, and a calm and imperturbable temper, and drew up abstracts of the papers read at the meetings with a precision that might stand in comparison with those of Dr. Wollaston at the Royal Society." As an instance of character it is mentioned that in early life he had been a minister of the Unitarian persuasion, but resigned his cure on conscientious grounds. He was a corresponding member of the Academy of Dijon, &c. He died in London, on the 15th April, at the advanced age of eighty.

Dr. Stanger, the able and energetic naturalist of the ill-fated Niger Expedition, was born at Wisbeach, in Cambridgeshire, in 1812. He took his degree of Doctor of Medicine at Edinburgh, and subsequently visited Australia. He afterwards superintended, under the direction of the Government, the construction of roads near Cape Town, then returned to England, and settling in London, commenced the practice of his profession.

But the pursuit of natural history had greater charms for his enterprising character. In 1841 he joined the Niger Expedition under Captain H. Trotter, R. N., and was one of the few of that gallant but unfortunate band who were not struck down by the devastating fever of the country. It was mainly owing to his energy, assisted by Dr. M'Williams, that one of the steamers was brought down the river. In 1845 he was appointed Surveyor-General to the new colony of Natal, where, with the exception of a short interval of two years passed at home, he continued until his death. In this young colony his time was spent between the conscientious discharge of the duties of his office and a zealous investigation of the natural history of the district. But the pressing calls of his official duties did not permit of his reducing to order his many observations on natural history. One of his last contributions to botanical science, to which he was particularly devoted, was the discovery of a plant belonging to the family of the Cycadeæ, combining many peculiar characters, and named after him *Stangeria*. There is now a plant of it in the Royal Gardens at Kew, producing fruit. Exhausted by fatigue and cold, after travelling from Maritzburg to Natal on horseback, he died on the 21st March, 1854, and was honored with a public funeral as a mark of the respect in which he was held by the

authorities and inhabitants of the district. His loss is the more to be regretted, inasmuch as it disappoints those hopes held forth by my predecessor last year, in allusion to the geological discoveries to be expected from Dr. Stanger, who was to have undertaken an official geological exploration of the province of Natal.

* * * * * * *

The only loss we have sustained amongst our Foreign Associates is that of Dr. Gotthelf Friedrich Fischer de Waldheim, Professor of Natural History in the University of Moscow. He was born at Waldheim, in Saxony, on the 15th October, 1771, and studied mineralogy at Freiberg, with Leopold von Buch and Baron von Humboldt, completing his medical studies at the University of Leipzic. At Paris he subsequently attended the lectures of Cuvier, and carefully studied the natural-history collections of the French Museum. He had already given evidence of his extensive learning by numerous publications, when, in 1800, he was appointed Professor of Natural History at the Central School of Mayence. On his arrival there, however, he found that the chair had been given to another; and with that power of adaptation which belongs to true genius, he at once accepted the office of Librarian, which for a time led him away to other studies, particularly typographical antiquities. On this subject he published several valuable works until 1804. But he did not, in the mean time, neglect his favorite pursuit; he founded at Mayence a Natural History Society, of which he became the Secretary, and in 1804 published his 'Anatomie der Maki un der ihm verwandten Thiere.' In the same year he was appointed Professor and Director of the Museum of Natural History at Moscow, where a new field was opened to his talents, in which he had labored with zeal and energy during the remainder of his life. In the year 1805 he founded the Society of Naturalists of Moscow, and published the first volume of his 'Description du Muséum d'Histoire Naturelle,' the copper-plates of which he engraved with his own hands. This Museum, for the establishment and improvement of which he had so strenuously exerted himself, was destroyed during the conflagration of the city in 1812. Such a calamity would have gone nigh to overwhelm an ordinary man. Dr. Fischer rose above the circumstances, and with redoubled ardor immediately set to work to replace, as far as possible, the treasures which had been lost. Such were his efforts, and such was the success with which they were attended, that in a very few years the new Museum had again acquired a valuable collection of objects of natural history. He had now begun to direct his attention more exclusively to the study of fossil zoology, or as it is now called, Palæontology. In the 'Bibliographia Zoologiæ et Geologiæ' of Agassiz, published by the Ray Society, there are no less than 150 notices of separate works and memoirs in Journals and Transac-

tions published by him during the course of his long and laborious life. Among these are many bearing directly on our science, and which must have had considerable influence in directing the attention of the Russian Government to the mineral riches of the country, and of making its geological features better known beyond the limits of his own district, I will only mention a few of his more important works:—"Oryctographie du Gouvernement de Moscou," 1837; "Bibliographia Palæontologica Animalium Systematica," 1810; a second edition in 1834; "Notice des Fossiles du Gouvernement de Moscou," 1809–1811; "Notice sur quelques Animaux fossiles de la Russie," 1829; "Ueber verschiedene fossile Elephanten-species, die man unter dem Namen Mammouth begreift," 1831; "Recherches sur les Ossemens fossiles de la Russie," 1824; "Lettre à Murchison sur le Rhopalodon, genre de Saurien fossile du Versant occidental de l'Oural," 1841; Revue des Fossiles du Gouvernement de Moscou," 1846; and many others. He was elected a Foreign Member of this Society, and of the Linnean Society, in 1820. He died at Moscow, on the 6th of Oct., 1853, having nearly completed his eighty-second year.

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ART. XXXVI.—*Notice of Fossil Bones from the Red Sandstone of the Connecticut River Valley;* by JEFFRIES WYMAN, M.D.

THE fragments of bones here described were noticed more than twenty-five years since by Mr. Solomon Ellsworth while blasting for a well in East Windsor, Ct. The well had been sunk to the depth of twenty-five feet below the surface and eighteen feet into the sandstone, when some pieces of bone were recognised among the debris. They were examined by Prof. Nathan Smith and his account of them with a note by Prof. Silliman may be found in Vol. ii, of this Journal. In Vol. iii, p. 247, in a letter to the editor by John Hale Esq., it is stated that Dr. Porter who saw them when they were removed from the well, decided that they were not, as they had been supposed to be by some, human, but "belonged to some animal and that the animal must have been about five feet long. The tail bone was easily discovered by its numerous articulations distinctly visible, and by its projecting in a curvilinear direction beyond the mass." He also says that "they resemble some particular bones of the human body but would also compare with certain bones of other animals," but does not state what bones or what animal.

Through the kindness of Prof. Silliman and of Mr. Alfred Smith of Hartford, Ct., I have had all the specimens now remaining of the above mentioned collection placed at my disposal for examination. The collection consists of about sixteen pieces

of sandstone each containing one or more fragments of bone, but the larger number of these are so much broken up, that it was found impracticable to determine anything with regard to their nature. All are soft, chalky and friable; and in no instance is there one entire bone or one complete articulating surface visible. Yet imperfect as they are, coming from the New Red Sandstone, anything which can be determined with regard to any of them is of great interest. For while we have well preserved the innumerable impressions of the feet of birds and reptiles, the bones here noticed are, in so far as I can learn, the only ones which are publicly known to have been brought to light.

One of the best preserved bones is the fragment of a *vertebra*, the body of which is nearly entire, and is probably one of the "tail bones" described by Dr. Porter, but according to his description must have been more complete than it now is. The arch is broken off; the base of one pedicle remains; a transverse process on the left side is indicated by an imprint in the matrix; this process is broad at the base, thin and triangular, projects laterally in a horizontal plane and is slightly recurved. One extremity of the body is so completely imbedded in the matrix that only a slightly convex border is visible; the other extremity is exposed but is obliquely fractured, leaving only a part of the natural surface visible, and this is concave; the body is constricted in the middle, having an hour-glass shape, has no indentations on the side, and is somewhat compressed laterally. The floor of the spinal canal is represented by a deep channel, the sides of which meet at the bottom at an acute angle midway, but at either end spread out and become nearly level. The dimensions of the vertebra were as follows,

Length of body, ...	1·00
Breadth of body at extremity, ...	0·55
Breadth of body in the middle, . . .	0·30
Length of transverse process (imperfect), . .	0·60
Breadth of transverse at base, ...	0·52
Breadth of spinal canal at narrowest part, .	0·10

In front of the body just described is the fragment of what appears to be a second vertebra, and beneath this is a pointed fragment about seven-tenths of an inch in length, bifurcated where it touches the other bones and which has the character of an inferior spinous process. Behind the more complete vertebra is the anterior portion of a *third*, obliquely fractured so that a portion of its concave anterior extremity, and the edge of the left transverse process alone remain. In another piece of matrix is a portion of the arch and a cast of the spinal canal of a *fourth* vertebra, having also a portion of the spinous process; the canal conforms to that already described, and in front of this last vertebra is the fragment of the spinous process of a *fifth*. In another piece of

matrix is the fragment of a *sixth* having the articulating processes imperfectly preserved.

Of these six imperfect vertebræ only that first described, and the piece behind it offer distinctive characters of any importance. The presence of an inferior spinous process shows that it belonged to the caudal series; the existence of an anterior concavity, and the presence of a superior transverse process coexisting with an inferior spinous process are essentially reptilian features. In fishes the superior transverse processes are always deficient, and the inferior ones alone exist; these in the tail bend down to form the inferior spinous processes. If this bone is the one referred to by Dr. Porter as a "tail bone" his view as regards the kind of vertebra was correct, though he gives no reason for his conclusion. In addition it may be stated that it is a caudal vertebra of a Saurian reptile, to which it corresponds in the shape of the body and the transverse processes, and more nearly to those of the Crocodiles than any others.

Nearly all the remaining bones are too much broken up to allow of the determination. Still some of them present features of a general character, which are of sufficient significance to require a few remarks. One of the most striking characteristics is that of hollowness, and this in two cases is quite remarkable. In one of them we have a transverse fracture of what appears to be a cylindrical bone; the exposed fractured end is oval, measures one and one tenth of an inch in its long and six tenths in its short diameter; the cavity is very large, without cancelli and the walls of the bone are only one tenth of an inch in thickness. Near to this in the same mass of the matrix is another fragment which is exposed in transverse section as well as on a portion of its lateral surface. In its transverse section, it is more compressed than the preceding, but has equally thin walls and no cancelli. The whole fragment bears some resemblance to the upper extremity of the humerus of a bird,* distorted by pressure. The large size of the cavity and the thin walls lead us to compare this and the preceding specimen with the remains of birds, for it is among these alone that the cavities become proportionally so large and the walls so thin. Still it must be remembered that some reptilian bones are hollow. The Iguanodon, Pelorosaurus and Hylæosaurus described by Dr. Mantell, which had medullary cavities in the humerus and femur, but in them the walls were proportionally very much thicker, and the medullary canal was quite short.

Several other pieces of matrix contained fragments of bone which were hollow and with thicker walls; but they were too much injured to allow of determination.

* Its resemblance to a humerus was noticed by Prof. Silliman in the note appended to Mr. Smith's communication—in vol. ii, of this Journal

Two more pieces of bone remain to be noticed: they are about three inches in length, are longitudinally fractured and a large portion of each is gone; they have some indications of cancellated structure near one of the ends, and bear some resemblance to the bones of the fore-arm of a Saurian reptile, the form of the longer bone corresponding with that of the ulna, and the lower extremities of both agreeing in proportions with an ulna and radius; the upper portion of the supposed radius is nearly all gone.

ART. XXXVII.—*On the Distribution of Rain in the Temperate Zone;* by H. W. DOVE.*

IN the Meteorological Annual of France for 1850 observations for ten years on the quantity of rain falling in Algiers are given by Don, which show that the quantity diminishes almost regularly from January to July, and then regularly increases to December. This regularity is seen in the number of falls of rain, for in these ten years there were in January 88 rainy days, in December 83; in July, on the other hand, only a single one in 1844. These proportions hold good for the Canaries and Azores, they apply, too, even in the south of Europe, for in Funchal the quantity of rain diminishes from 92‴ in January to 0‴·9 in July. In St. Michael it is found after ten years of observation, to be four times greater in January than in July. In Lisbon the proportions are for December and July 55 : 2, in Palermo 37 : 2½, Naples has in March and October 49‴, in July not 7‴. Even in Rome the fall of rain is ten times more in October than in July. This periodical law is seen not only in the measurement of the fall of water, but in every accompanying phenomenon of temperature. After three months of almost perfectly clear weather only rarely interrupted by a tempest, the rains begin in Rome, on the 10th of October, though often sooner, and last, with fierce storms, almost without interruption, till the end of December. They diminish a little towards spring, so that the whole winter is rather a changeable than cold season, a continuous change from Tramontane to Sirocco. Even if we cannot divide the year, as do the Indians on the Orinoco, into a season of sunshine and clouds, yet the contrast of the rainless hot months and the continuously rainy winter is very marked. The beginning and end of this rainy period are usually marked by storms; Lucretius says of it:

> "Auctumnoque magis, stellis fulgentibus apta,
> Concutitur cœli domus undique, totaque tellus;
> Et, quom tempora se veris florentia pandunt:
> Frigore enim desunt ignes, venteique calore
> Deficiunt, neque sunt tam denso corpore nubes.

* Annalen der Physik, No. 94. Translated for this Journal by Dr. Rosengarten.

Inter utrasque igitur quom cœli tempora constant,
Tum variæ causæ concurrunt fulminis omnes.
Nam fretus ipse anni permiscet frigus, et æstum;
Quorum utrumque opus est fabricanda ad fulmina nobis,
Ut discordia sit rerum, magnoque tumultu
Ignibus et ventis furibundus fluctuet aër.
Prima caloris enim pars, et postrema rigoris,
Tempus id est vernum: quare pugnare necesse est
Dissimileis inter sese, turbareque mixtas.
Et calor extremus primo cum frigore mixtus
Volvitur, auctumni quod fertur nomine tempus:
Hæic quoque confligunt hyemes æstatibus acres.
Propterea, sunt hæc bella anni nominitanda."—*Lucretius*, VI, 357.

This description, which is exact for Italy, little suits our climate, the greatest number of rain-showers and the most storms occurring with us in the mid-summer months. But since the beginning and the end of the rainy period in Italy are marked by strong southerly winds, and a number of rules for the weather have come down from the olden time, which have their importance only over limited geographical distances, it hence seems to me not unlikely that the well-known equinoctial storms may still be admitted by us although in spite of the rule, we look in vain for them in our bright quiet September, and are not satisfied with the astronomical explanation of the postponement of the autumnal equinoctial to November or December.

The first explanation of this phenomenon is given by L. von Buch in his observations on the climate of the Canary Islands. "The Canary Islands," he says, "feel nothing that corresponds to the tropical rains, those rains, that is, which according to the sailors' phrase, follow the sun and fall when the sun reaches its greatest height. The rains of this climate first appear when the temperature falls considerably in winter, and the difference from the climate of the equatorial regions thus becomes greater and more strongly marked. The cause of these rains then appears to be the same that acts even to the pole,—the cooling of the warm air and the associated vapor which comes in from the southwest out of tropical regions and the lower latitudes, then, since this air in the autumn temperature of the Canary Islands is hardly cooled down to the condensation point of vapor, it is easy to understand why the rains begin much later here than in Spain or Italy, and yet earlier than in France and Germany. Rain is rare on the sea-shore before the beginning of November and does not often continue later than to the end of March. In Italy this period of rain lasts from the first half of October to the middle of April.

"Especially instructive and important for meteorological science," continues L. von Buch, "is the way in which the northeast trade wind is moved in winter by the southwest winds. These do not begin in the south and go towards the north, as would at first be imagined from their direction; but they reach

the Portuguese coasts sooner than Madeira, and Madeira sooner than Teneriffe and Canary; and so, as if from the north, these winds come gradually from above downward; and in these higher regions they are found even during the summer, while the north-east wind is moving over the face of the sea with considerable force. These higher winds descend slowly from the elevated atmosphere of the mountains. This is seen clearly in the clouds which in October surround the point of the South-Peak; which fall continually lower, and at last settle on the crest of the hills some 600 feet high, between Orotawa and the south coast, here to break out in fearful storms. Often a week, and sometimes more passes before they are felt on the seashore, for they last here for months. Rain falls on the outlying spurs of the mountains, and the Peak is covered with snow."

I consider the views thus expressed by von Buch, among the happiest thoughts which were ever brought forward in the science. It is the more to be regretted that in the year 1829, in his 'Essay on the Subtropical Zone,' L. v. Buch should have given these views an extent not sustained in nature. I know very well that formerly all naturalists supposed that the phenomena observed within circumscribed geographical limits, stretched similarly over the whole extent of the earth;—that they looked upon the monsoons as a local modification of the tradewind only within the Tropical Zone;—that they had no idea of finding in the Temperate Zone similar extensive differences under different longitudes, and of like geographic extent. I am far from charging von Buch with having created a subtropical zone that does not exist as a zone. Now however, that extensive observations over the whole surface of the earth are opening to us regions which were formerly beyond the reach of inquiry, it would be unjustifiable to blind ourselves to the new points of view that are offered us. It might sound paradoxical to say, that in summer the interior of Siberia has really such an absence of wind currents as we are in the habit of regarding as confined to the equator, but it must be declared a repudiation of physical laws, to say that the air of the Temperate Zone continually moves around the earth like a steady west wind, as is now printed in every text-book, for every one should know that the air moves only towards a region of disturbance, or after rising to such a place then turns back.

As proofs of the subtropic zone, Leopold v. Buch points out the yearly periodical diminution of atmospheric pressure from summer to winter, and deduces this diminution from the currents coming down from the mountains. Since in the Indian Ocean the southwest monsoon causes the barometer to fall, he takes this as one of the descending winds; for he says: In India, the southwest monsoons lower the barometer, and this just in proportion as they move downward. He adds further, that this periodic barometric

change does not extend across the Himalayah range, and that over these mountains another meteorological system begins; he extends this zone, making it to embrace the whole earth, beyond America to the Sandwich Islands.

In 1831 I wrote as follows, opposing the notion that the southwest monsoons are winds descending from above: (vide Pogg. Ann., xxi, 177,) "all observation seems to show that between the southwest winds on the outer limits of the monsoon and those in the Indian Ocean, the following differences exist: the former appear in winter, the latter in summer; those in a southerly direction are bordered by northeast winds, the others by southeast winds." According to Halley's theory, as followed by Mushenbroeck, Capper, Hube and Horsburg, these lower currents do not come from above downward, as conjectured by Halley, and proved by v. Buch.

The question why the appearance in the Atlantic is so different from that in the Indian Ocean, I then tried to answer by the theory that the high lands of Asia intercepted the passage of northerly currents, and thus no other current met the monsoon as it hurried up from below, except the mass of air between the neighborhood of the calms and those high mountains. With the approach of the sun, the mass of air moving in the perpendicular circuit diminishes, till at last it no longer opposes the southeast wind and takes quite an opposite direction. In the explanations of a phenomenon, all views may be divided into two classes, actual explanations, and evasions of nature. Of the latter kind was my former explanation, and so conscious was I of its weakness, that when the first Journal of Observations in Siberia appeared, I recognised and expressed the importance of its facts with reference to the theory of monsoons, pointing out that the barometric oscillation, which L. v. Buch thought was stopped at the Himalayah was greatest in the heart of the Asiatic continent, and that therefore the point of derivation of the southerly currents lay far towards the north.

Although the universality of the phenomenon of periodic diminished pressure was so clear that Kupffer in the edition of Asiatic Observations published in 1841, said: "one has only to look at the results of the observations contained in this volume to find a complete confirmation of all that M. Dove has advanced on this point," yet I then first proved decidedly that the southwest monsoons are the lower tradewind, which rushes toward the point of disturbance to fill up the void that owing to the increasing temperature here in inner Asia the diminished moisture was not sufficient to occupy. The proofs are indirect indeed, but all who have occupied themselves with meteorological observations know that hardly any problem of this nature can be directly proved, and that whoever has to do with the winds must follow the vane.

In a treatise that appeared in the Phil. Trans. for 1834, "On the Atmospheric Tides and Meteorology of Dukhun," Colonel Sykes showed that on the Plateau of Deccan, only 23″ of rain fell, which is about twenty per cent of the quantity at Bombay; but that the quantity is much more considerable on the sides of the mountain than in Bombay; and according to new observations in Mahabuleshwur, in fourteen years it amounted to the enormous quantity of 250″. In an essay contained in the Phil. Trans. for 1850, "Discussion of meteorological observations taken in India at various heights, embracing those of Dodabetta on the Neilgherry Mountains," he has further discussed the same phenomenon. Finally, last year's Journal of the Asiatic Society of Bengal contains observations at the military stations in Hindostan, from which it appears that in Cherrapoujee on the slope of Cossya Hills at the same height as Mahabuleshwur, there fell the unheard of quantity of 610 inches in one year, an amount far exceeding that of any of the more elevated stations. A quantity increasing so extraordinarily at a certain height, which decreases however in places still higher, is quite irreconcilable with currents of air descending from above; it speaks more for a current moving horizontally against the side of the mountain, which is compelled to ascend on it, and then loses its vapor. I find a second proof of this ascent, in the fact that, (as I gather from some new calculations of the temperatures of Hindostan,) all the high stations show on the approach of the southwest monsoon a sudden increase of temperature, which is not found in similar proportion at any station on the level of the sea, a proof that those heights are swept by warm winds out of the valley, which climb up on them, for certainly the temperature curve of the higher stations must be more even than that of the lower.

From this, the immediate consequence is that the summer rains of the monsoon have nothing in common with the subtropical rains on the north and west coast of Africa. With the tropical they agree in succeeding the highest point of the sun's ascension; but they differ in not succeeding a '*courant ascendant*,' but an equatorial current, which is cooled at a higher level. The direction of the air-currents too is quite opposite. The tramontane rules in summer over the Mediterranean as a retreating protracted northeast current, at the same time the southerly monsoon prevails over the Indian Ocean. When the northeast monsoon reaches here in winter, the sirocco rages in the Mediterranean.

In 1835 I showed that the proportions of rain in the middle and northerly portions of Europe are comprehended from this simple point of view, that the time of winter rains on the outer limits of the tropics, the further we go from them, separate into two maxima united by slight fallings off, which come together

in South Germany into one summer-maximum, when the period of temporary rainlessness entirely ceases. This is observed especially where the current in the course of the year rises and falls considerably, and therefore at places, in a word, near the outer limits of the current, sometimes within it, and sometimes without it. It is clear that where this is not the fact, the changes in the temperate zone must be different. The delay of the current in the annual period is thus clearly accounted for.

As early as 1675, *Seller* laid down the inner limits of the northeast current in the Atlantic Ocean, very different from *Horsburg* in the early part of the India Directory. According to him, the zone between the two currents is fifty miles broad in winter, and one hundred and twenty in summer. Lately, Maury has tried in his large atlas, to ascertain the change of the northeast current for each month. He has shown clearly that this change is greater on the African than on the American side, or as I had before inferred, from the barometric phenomena, that the limit of calms and the associated phenomena of the current do not move in parallel lines but like a swinging thread, having its node-point in the West Indian sea, where the trade wind is a constant wind, and its greatest breadth of swing in the Indian Ocean, where it turns into the monsoon. How these extremes pass in the interior of Africa from one to the other, that is, how the current becomes a monsoon, we do not know, since most African travellers think that they have quite paid their tribute to meteorology, by complaining of the long continued heat there.

(*To be continued.*)

ART. XXXVIII.—*Correspondence of M. Jerome Nicklès, dated Paris, Sept.* 1, 1855.

"*Universal Exposition*" *at Paris.*—With the exception of Russia, the Industry of all Nations is represented at the Paris Exposition, but some more imperfectly than others, and none so poorly as that of the United States. The various machines, pieces of apparatus and products of which I have spoken in my communications to the American Journal are there on exhibition.

The ether machine of M. Trembley* is among them. The boiler is open to view. It has been rendered perfectly tight, by plunging the system of tubes which compose it into melted bronze. The ether vapor is thus completely confined and escapes only by the natural openings.

Boutigny exhibits his small boiler, by which the surface of evaporation is augmented without increasing the heating surface.†

M. Franchot has there his hot-air machine, as it was made in 1836.‡

The Arithmometer of M. Thomas de Colmar§ is presented on a large scale, adapted to calculate 30 numbers—which is beyond the wants

* This Journal, Nov., 1853. † Ibid., May, 1853. ‡ Ibid. § Ibid, May, 1855.

even of science. It is a very fine instrument as large as a common piano; and beside it there are others so small that they may be taken in the arms.

The Thermogenic machine of Mayer and Beaumont* is the object of unceasing curiosity; and it certainly is a remarkable fact that in this way, the simple friction of a piston of leather within a tube, making 300 turns of rotation in a minute, should furnish steam. The boiler is ordinarily under 2-horse pressure. It is actually put in use within the Crystal Palace, where it furnishes the hot water required for making coffee and chocolate. To produce the heat requisite for 2 horse power in the steam, requires at least 4 horse power. The inventors propose to use their machine only when it is convenient to utilise lost force.

In the fine display, made by Nachet, there are the multiple microscopes, of which I have spoken.†

Duboscq has on exhibition his electric regulator; Edmond Becquerel, his apparatus for chromatic photography; M. Felix Bernard of Bordeaux, a refractometer and also an apparatus for determining the polarization of the atmosphere.

Duboscq shows also stereoscopes by reflection and refraction. The stereoscopic views are less successful than those in the English Department.

Among the articles from England, we observe with interest the apparatus employed by M. Tyndall in his researches on magnetic phenomena; also the electro-magnet employed by M. Faraday for diamagnetic phenomena, and for examining the effect of magnetism on polarized light; this electro-magnet is in the show-case of Ruhmkorff, where there is also a differential galvanometer of Weber, and Ruhmkorff's induction apparatus,‡ which has proved of great service to amateurs in electricity. There is also a model of circular electro-magnets;§ another of trifurcate electro-magnets;‖ the rheostat of Wheatstone, etc.

M. Froment exhibits many kinds of apparatus of great interest:—as writing telegraphs;—a telegraph with a finger-board;—the gyroscope of Foucault;¶ apparatus for determining the velocity of heat, light and electricity, which has had the approbation of Arago, Bréguet, Silbermann, Froment, Foucault, Fizeau, &c. He also exhibits an electro-magnetic machine, which it is true will not be found particularly useful, but is still interesting on account of the plan of its construction. It is a circle of brass carrying at intervals iron armatures. This circle is placed in a drum also of brass, but furnished with horseshoe electro-magnets. On putting the current in action each armature in turn is attracted; and thus the interior circle is put in motion and acquires a velocity of rotation on itself, which is quite fatiguing in consequence of the shocks to which the circle is exposed.

In the French department, there are two other electro-magnetic machines: one moved by circular electro-magnets, the other with trifurcate magnets. Six circular electro-magnets with three poles are arranged on an axis. Around these electro-magnets, are arranged six cylindrical iron bars serving as armatures, and moveable upon their axes.

* This Jour., September, 1855. † Ib., Jan., 1855. ‡ Ib., January, 1853.
§ Ib., Jan., 1853. ‖ Ib., Jan. and May, 1853. ¶ Ib., March, 1853.

The circumference of the electro-magnets is at intervals terminated by brass plates so arranged as to serve to interrupt the magnetic circuit. As long as the iron portion of the circumference approaches the armature, it tends to separate itself from it, and carries in its movement the whole mass of the electro-magnet with the axis which supports it and the fly-wheel on the latter; but at the moment when contact takes place, the current is interrupted and driven through the electro-magnet, in consequence of which the play just described is recommenced. The apparatus is neither powerful nor economical. It would be better to employ circular electro-magnets with two poles.

The electro-magnetic machine with trifurcate magnets is more powerful, since the action between the magnet and armature is not exerted at the time of contact. But I may describe these machines at a future time with some details, and now add only that neither of them resolves the problem of an economical motor.

The only galvanic battery on exhibition, which has any novelty, is that called the electro-hydro-dynamic, by M. Chenot, in which he uses the sponge of iron, of which he is the discoverer, as already communicated. The same inventor has a machine which is an interesting application of electro-magnetism to the dressing of iron ores. These ores, being magnetic, are thus easily separated from the gangue and made ready for the furnace. The machine is a drum carrying electro-magnets on its circumference. A screen carries the impure ore under the drum where the poles of the electro-magnets take up the oxyd of iron and leave the gangue on the frame to be transported by it into an enclosure one side. As the drum moves on in rotation, the galvanic fluid is made to leave the first range of magnets and act upon the next; the ore of the first is thus dropped, while the next is taking its load. M. Chenot thus uses with profit ores containing only 9 per cent. of oxyd of iron.

Among the other applications which are on exhibition, not to speak of the magnificent galvanoplastic products, there are different systems of electric telegraph.

There is the signal telegraph of M. Bréguet, in which the same signals are made as sometimes used in the ordinary aerial telegraphing. It is in fact only a union of two telegraphs with ordinary dial plates; for if we suppose two of these telegraphs in action at the same time, and so arranged as to produce by their indicating needles whose extremities move over a black line drawn on the dial plate of the receiver, 8 positions in its turn in place of 26 which the alphabet requires, you will have some idea of the signal telegraph which has been adopted by the administration of telegraphic lines in France—adopted not without resisting progress for several years, and denying the benefits of the electric telegraph for a long period after this invention was employed in the United States.

M. Froment, exhibits a telegraph in which manipulation is through a box with a finger-board, having as many keys as letters. A arbor in this box tends constantly to move under a clock movement and carries in its motion the wheel which opens or closes the circuit; but it is retained by a catch which pressure on a key raises,—a simple arrangement for producing the breaks in the current. The receiver is that of the primitive telegraph.

The forms of telegraphic apparatus are numerous and interesting, though constructed on known principles. The more important are those of Froment, Bréguet, Garnier, in France, Berg and Soerrenden, of Sweden; the Company of the Eastern Railway, etc. The bell telegraph of Mirand has now many applications: its principle is that of Morse's telegraph.

Other examples of the application of electricity, are the Anemograph of Dumoncel, an instrument for measuring the direction, elevation, and force of the winds;* a heat regulator for hot-houses and drying chambers; and a system of electric communication between railroad trains.

M. Henley of London, has on exhibition a magneto-electric machine made of powerful magnets for springing mines. Deleuil, Breton, and Bekking of Holland, have induction apparatuses for medical uses. There is nothing especially remarkable in them.

The exposition contains also models of the submarine cables which have been used in connection with telegraphs, and of those which are soon to be laid down between Italy and Algiers.

In another communication, I propose to allude to other machines and chemical products and arts represented at the Crystal Palace.

Zoological Society of Acclimation. Project of founding a Museum of Applied and Comparative Natural History.—This zoological society continues its labors and with important results. Being essentially pacific in its objects, it has entered into relations with the Imperial Economical Society of St. Petersburg, and established with that Society a system of exchanges of objects industrially useful.

This Russian Society has in view an end long since proposed for France by M. Guérin Meneville, that is to make a collection of all raw products raised and used in the country, and of specimens illustrating the series of transformations they undergo in the processes of the arts. It would include also objects injurious to the articles which man cultivates. In such a gallery, the agriculturist, the trader, and the practical man, will find whatever illustrates his own departments, and be able to make comparisons important to direct them in the various operations of the arts and trade.

The St. Petersburg Society has already made such a collection of the productions of Russia, and their transformations. It includes all kinds of wheat, from the grain to the flour, bran, and starch, &c., as well as models of machines employed in cultivation, &c., from the plough to the wind-mill and the implements of baking. So there is wool of various kinds, and the raw material, and all the tissues coarse and fine, &c.; and so with a multitude of objects, pertaining to the three kingdoms of nature.

The Zoological Society of Acclimation, not wishing to confine itself to the productions of France alone, has resolved to reserve the objects it may obtain by exchange for a "Muséum d'Histoire Naturelle appliquée et comparée," to contain the products of all countries so as to exhibit the differences between them.

Acclimation of the Angora goat.—The Society is trying to decide whether the Angora goat can be acclimated. At their request, they

* This Journal, September, 1853.

have recieved from Abd-el-Kader, at Broussa (Asia Minor) a flock of goats from Angora, fifteen in number, which have been sent to the mountains of the south, centre, and east of France. We have seen some of them in the enclosures for acclimation of Dr. Sace at Wesserling (Haut Rhin), a valley possessing much that is curious in its industry, and of interest in the remains of ancient glaciers still traceable there. Two kids have been born since the goats were sent to Alsace, and as yet there is nothing to show that acclimation is impossible. The orientals doubt the success, and say that the texture of the wool changes even for small distances in the same zone and the same region.

The region occupied by the Angora comprises a part of Central Galatia and a southwest portion of Paphlagonia, between the parallels of 39° 20′ and 41° 30′, and the meridians of 33° 20′ and 35° east of Paris, the surface of which is about 2350 square metric leagues. The goat avoids the highest mountains, not ascending beyond 1600 meters, according to M. Tchihatcheff, who has studied the animal in Asia Minor. It also keeps out of the lower valleys where the heat is high in summer. The village of Angora is the place where this goat is raised with the most success and in largest numbers. Its altitude is about 1120 meters and its climate is liable to great extremes.

One of the most striking characteristics of the Angora goat, is its strong attachment to its native soil: a removal, however slight, causes a change in the quality of the wool. M. Tchihatcheff observes that all attempts to transfer it to Constantinople, Smyrna, and other parts of Asia Minor, have been without permanent success, the wool of the second generation beginning to deteriorate.

We may hope from the success experienced in other cases of acclimation, that Asia Minor is not the only place where the Angora may be made to live and flourish. The Egyptian goose was brought to France from Egypt in 1799 by Geoffroy, senior, and his son finally succeeded in acclimating it.

Since these trials with the Angora were begun, M. Graells, director of the Museum at Madrid, has stated that a flock of one hundred of these goats was introduced into Spain in 1830, and that there are now two hundred of them in the mountains of the Escurial, and still another flock in the mountainous region of Huelva.

Academy of Sciences.—The universal exposition at Paris has had some effect on the Academy of Sciences in bringing distinguished men from all parts of Europe and Britain to its meetings. At one recent session, there were present MM. Liebig, Brewster, Wheatstone, De la Rive, H. Rose, Poggendorff, Rammelsberg, Dove, Steinheil, etc. At this Scientific Congress, the United States is worthily represented by Mr. T. S. Hunt, of Canada, whose numerous confining occupations as a member of the Jury at the Crystal Palace, have hindered him from pursuing his scientific labors. He has made a series of communications to the Academy of Sciences, some of them treating on theoretical subjects of a profound character connected with great questions in philosophical chemistry now commencing to occupy the scientific world, and others specially on the geology of Canada.

Bibliographical Notices.—Astronomie Populaire, d'Arago, vol. ii, Paris: Gide and Baudry.—This volume is almost wholly devoted to the

Sun, the Zodiacal light and Comets. The theory on the physical constitution of the Sun; a careful essay on the Solar Spots; an examination of the Zodiacal light; a very complete treatise on Comets, give great interest to this volume. The author discusses the question whether the Sun is inhabited, and treats of the magnetic effects of the Sun on the compass.

He also describes the parallactic stand, the equatorial and the rotating dome of the Paris Observatory, as also the Polariscope lens of his invention.

Etudes et Lectures sur les Sciences d'Observation et leurs applications pratiques, by M. Babinet, Member of the Institute. 1 vol., 12mo. Paris, Mallet Bachelier. Price 2½ francs.—This work is intended for popular reading. It contains several lectures, some of which were delivered at the periodical sessions of the five Academies of the Institute. One is entitled *Extraordinary Movements of the Sea*; another, *Comets in the nineteenth century*; another, *The Electric Telegraph*, in which M. Babinet states that he does not believe in the possibility of uniting Europe and America across the Atlantic Ocean, and thinks the course by Behring Straits the only one practicable; another, *On Astronomy in* 1852, 1853; *Aerial Perspective*; *The Stereoscope and binocular vision*; and finally it closes with an interesting chapter entitled "*Voyage dans le ciel.*" M. Babinet, a relative and friend of Arago, understands how to throw into his lectures the same charm and simplicity that characterised the popular public addresses of Arago.

Etudes sur la géographie botanique de L'Europe et en particulier sur la végétation du plateau central de la France; by M. H. Lecoq, Professor of Botany in the Faculty of Sciences of Clermont. 4 vols., 8vo, with plates. Paris: Baillière.—The author, who has for thirty years been exploring the mountains of Auvergne and the central plateau of France, has been led to compare the facts observed there by himself with those recognised by him in other regions. No one understands Auvergne like M. Lecoq; to him all questions are referred relating to the geology of that country.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the relations between the boiling points, specific volumes and chemical constitution of bodies.*—Since the appearance of his remarkable paper on the specific volumes of fluids, already noticed in this Journal, Kopp has published an elaborate memoir on densities, dilatations, and boiling points, confining himself however to a statement of his numerical results. In a still later publication he gives a brief summary of his conclusions which will be interesting to all chemists. With reference to boiling points the author finds a great number of facts which go to confirm the laws previously discovered by himself and already widely known. In a great number of comparisons of similar fluid bodies, the differences between the specific volumes taken at the boiling

points are found to be proportional to the differences between the formulas; thus the specific volumes of two different fluids differing in their formulas by $x.C_2H_2$ differ by nearly $x.22$. Isomeric fluids are found to have equal specific volumes. In numerous cases in which oxygen replaces an equivalent amount of hydrogen, the specific volume remains *nearly* the same as before the substitution. There appears however in these cases to be a real though small increase in volume. On the other hand, however, carbon can replace hydrogen in fluid compounds without producing a change of volume. The new researches confirm the results already obtained, that it is impossible, in a general manner, to deduce the specific volume of a liquid from its empirical formula, without regarding the direct results of experiment as inaccurate. It became therefore necessary to return to hypotheses on the rational constitution of chemical compounds and then the results show the uncertainty of the premises. The views brought forward can therefore only be regarded as presenting the simplest possible general expressions for the specific volumes of liquids. For this purpose Gerhardt's classification appears to be an advantageous one; the author's mode of representing the volume of the elements in fluid compounds is the same as that already employed by him (Ann., vol. xcii, 1,) but the numerical values of the specific volumes of carbon, hydrogen and oxygen are somewhat different from those formerly determined.

The specific volumes of fluids, which may be considered as derived from hydrogen $\left.\begin{matrix}H\\H\end{matrix}\right\}$ as type, correspond very well with the values deduced from observation if we take the specific volume of carbon as 5·5, that of hydrogen also as 5·5, and that of oxygen as 6·1.

The specific volumes of those fluids which may be considered as derived from water $\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$ as type, may be satisfactorily deduced by giving the values above mentioned to carbon and hydrogen, but attributing to the oxygen, when it stands in water, as a specific volume of 3·9: 2 eq. of oxygen within a radical have the specific volume $2\times6.1=12.2$, but 2 eqs. of oxygen outside of a radical have the specific volume $2\times3.9=7.8$. The same assumptions serve for compounds which may be deduced from multiples of water as types. For the determination of the specific volumes of compounds containing only carbon, hydrogen and oxygen, the author employed his own determinations, but for those of compounds containing other elements he made use of the observations of Pierre.

Sulphur occurs in its compounds in different ways, sometimes (1) replacing the oxygen in the type $\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$ as in mercaptan, &c., sometimes (2) replacing carbon within a radical, as in sulphurous acid compared with carbonic acid, sometimes (3) replacing the oxygen within a radical, as in sulphocarbonic acid compared with carbonic acid $C_2O_2\left\}O_2=C_2S_2\right\}S_2$. In the first and second case the specific volumes of the sulphur compounds correspond satifactorily with the observations if we take the volume of sulphur as 11·3, but for the third case the specific volume of sulphur appears to be greater, namely, 14·3.

The specific volumes of a great number of chlorine compounds correspond well with those deduced by observation, if we assume the specific volume of chlorine as 22·8. In like manner, the volumes of iodine and bromine appear to be respectively 37·5 and 27·5. It is remarkable that bromine, as the author formerly conjectured, has the same specific volume, in its compounds at their boiling points, which it has at its own boiling point in the free state.

The specific volumes of but few compounds of other elements have been so far studied as to admit of accurate determinations of the volumes of the elements themselves. Phosphorus, arsenic and silicon appear to have the same specific volume, namely, 26. The specific volumes of $SnCl_2$ and $Ti\,Cl_2$ are equal, and suggest that tin and titanium have the same volume in their fluid compounds.—*Ann. der Chemie und Pharmacie*, xcv, 121.

2. *On a new class of Organic Radicals.*—Wurtz has succeeded in obtaining a number of double organic radicals, the existence of which had long been inferred, but which other chemists had not been able to produce. The author's investigations were undertaken in order to throw light upon the question whether the radicals methyl, ethyl, &c., have when isolated the same equivalents which they have in combination, or whether, as maintained by Gerhardt and Laurent, these equivalents must be doubled. If the molecules of ethyl, for example, on being set free unite with each other so as to form double molecules such as $\left.\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}$ then we should expect to be able to replace one eq. of ethyl in such a compound by one of some other similar radical, such as methyl for instance, so as to have a mixed or double radical like $\left.\begin{matrix}C_4H_5\\C_2H_3\end{matrix}\right\}$. Wurtz has obtained such compounds by two different processes. 1st, by decomposing by sodium an atomic mixture of the iodids of the two radicals. 2d, by the electrolysis of a mixture of the fatty acids of the series $CnHnO_4$. The author in the first place gives an account of the properties of butyl and amyl as prepared and examined by himself. Butyl is a colorless oily liquid which boils at 106° C.: its density is 0·7057 at 0° C. and the density of its vapor 4·070=4 vols. Amyl (long since isolated by Kolbe) is a limpid and mobile liquid, having a slightly aromatic odor; its density at 0° C. is 0·7413: it dilates strongly on passing from 0° to 20° when its density is only 0·7282. The density of its vapor is 4·956=4 vols.: it boils at 158° C. Amyl deflects the plane of polarization to the right, but its power of rotation is very different in different specimens, and the author finds that the same is the case with amylic alcohol, which, however, deflects the plane of polarization to the left. By the action of perchlorid of phosphorus upon amyl, Wurtz obtained two chlorinated products having the formulas $\left.\begin{matrix}C_{10}H_{10}Cl\\C_{10}H_{10}Cl\end{matrix}\right\}$ and $\left.\begin{matrix}C_{10}H_9Cl_2\\C_{10}H_9Cl_2\end{matrix}\right\}$.

The radical ethyl-butyl as obtained by the first of the processes mentioned above is a light and mobile liquid, boiling at 62° C. Its density at 0° is 0·7011; the density of its vapor is 3·053=4 vols. Its formula is $\left.\begin{matrix}C_4H_5\\C_6H_9\end{matrix}\right\}$. Ethyl-amyl has the formula $\left.\begin{matrix}C_4\ H_5\\C_{10}H_{11}\end{matrix}\right\}$, boils at 88° and

deflects the plane of polarization to the right. The other radicals obtained are butyl-amyl $\left.\begin{matrix}C_8\ H_9\\ C_{10}H_{10}\end{matrix}\right\}$, butyl-caproyl $\left.\begin{matrix}C_8\ H_9\\ C_{12}H_{11}\end{matrix}\right\}$ and methyl-caproyl $\left.\begin{matrix}C_2\ H_3\\ C_{12}H_{11}\end{matrix}\right\}$. The author concludes his interesting memoir with many very ingenious and attractive theoretical suggestions, for which, however, we must refer to the original.—*Ann. de Chemie et de Physique*, xliv, 275.

3. *On organic compounds containing metals.*—Frankland, to whom we owe much of our knowledge of this interesting class of bodies, has published an extended account of the results of his investigation of zinc-ethyl, which is doubtless one of the most remarkable of this class of bodies. To prepare this substance upon a large scale, the author employed metallic vessels (iron or copper) capable of resisting a strong pressure, in which the materials could be heated directly or after they had been placed in glass tubes. In this manner four to five ounces of zinc-ethyl could be prepared at a single operation. In one of these vessels granulated zinc and iodid of ethyl mixed with ether were placed, both dried with extreme care: the whole was then heated for 12 to 18 hours in an oil bath to 130° C. After opening the apparatus the contents were distilled off, the receiving vessels being filled with dry hydrogen, nitrogen or carbonic acid. Zinc-ethyl at ordinary temperatures is a colorless transparent very mobile fluid, of high refractive power and peculiar rather agreeable, smell. Its density is 1·182 at 18° C.: it does not freeze at −22°; it boils at 118°, and distills without decomposition. The density of its vapor was found to be 4·259. It consists therefore of 1 vol. of zinc vapor and 1 vol. of ethyl gas, the two condensed to one—

1 vol. ethyl gas,	2·0039
1 vol. zinc vapor,	2·2471
1 vol. zinc ethyl,	4·2510

The condensation in this case is very remarkable and seems to lead to the conclusion that the combining vol. of the vapor of zinc is not 2 like that of hydrogen. The author suggests that its combining vol. may be 1 like that of oxygen, and that zinc-ethyl vapor may therefore consist of 2 vols. of ethyl gas and 1 vol. of zinc vapor, the 3 condensed into 2 as in water. There is no instance known in which equal volumes of two gases combine with condensation. It appears therefore extremely probable that zinc and the metals belonging to the same group do not correspond to hydrogen as heretofore assumed.

Zinc-ethyl, though endowed with very powerful affinities, does not appear to be capable of forming true chemical compounds with electronegative elements. In the air it takes fire and burns with a brilliant blue green-edged flame, evolving dense vapors of oxyd of zinc, water and carbonic acid being simultaneously formed. When, however, zinc-ethyl is mixed with ether and a stream of oxygen passed into the mixture, the gas is absorbed and a white precipitate soon separates. The products of the oxydation here were found to be ethylate of zinc, acetate of zinc, and hydrated oxyd of zinc. Iodine acts powerfully upon zinc-ethyl, the reaction being represented by the equation

$C_4H_5Zn+2I=C_4H_5I+ZnI$. Bromine acts explosively upon zinc-ethyl, with formation of similar products. In chlorine, zinc-ethyl takes fire, the zinc and hydrogen burning, while carbon is set free. Dry flowers of sulphur gently heated with an etherial solution of zinc-ethyl unite with both zinc and ethyl and form mercaptid of zinc, C_4H_5S+ZnS. Water and zinc-ethyl instantly decompose each other, the reaction being $C_4H_5Zn+HO=C_4H_5, H+ZnO$: similar decompositions occur with the hydracids.—*Ann. der Chemie und Pharmacie*, xcv, 28, *July*, 1855.

4. *On the constitution of the ethers.*—Bêchamp has presented to the Academy of Sciences a memoir on the action of terchlorid of phosphorus upon the alcohols and hydrated acids which possesses much theoretical interest. In a former paper the author shewed that glacial acetic acid behaves with terchlorid of phosphorus like a mixture of water and anhydrous acetic acid. In the present memoir he endeavors to show that alcohol is to be regarded, under certain circumstances at least, as a hydrate, and the compound ethers as salts of oxyd of ethyl. Very pure and dry acetic ether boiling at 74° C. heated in a sealed tube to 160°–180° C. during 24 hours gave a mixture of chlorid of ethyl, chlorid of acetyl and phosphorous acid. The reaction is here represented by the equation $3C_4H_3O_3, C_4H_5O+2PCl_3=2PO_3+3C_4H_3O_2Cl+3C_4H_5Cl$. Anhydrous acetic acid heated with the terchlorid of phosphorus yields chlorid of acetyl, the reaction being $3C_4H_3O_3+PCl_3=PO_3+3C_4H_3O_2Cl$. Very pure and dry ether heated with the terchlorid yields a similar reaction; we have $3C_4H_5O+PCl_3=PO_3+3C_4H_5O$. Hence the decomposition of acetic ether is precisely that of a mixture of ether and anhydrous acetic acid. The chlorid of phosphorus acts very powerfully upon absolute alcohol: the products are chlorhydric acid, chlorid of ethyl, phosphite of ethyl, and phosphite of water or hydrated phosphorous acid. The quantity of phosphorous ether is always very small, and it may be regarded as a secondary product, the alcohol being etherified by the muriatic acid. The equation will then be $3C_4H_5O, HO+2PCl_3=3HCl+3C_4H_5Cl+2PO_3$. The author concludes from these experiments that alcohol and the ethers of the monobasic acids are compounds of the same order and formed by the union of oxyd of ethyl with the anhydrous acid, since each one of the compounds which we suppose to exist in the alcohol or ether behaves as if it were isolated.—*Comptes Rendus*, xli, 23, *July*, 1855.

[Note.—It must be confessed that the author's reasoning is not very cogent, as all the reactions above mentioned are very easily explained by the so-called water theory, that is, by referring ether, alcohol, anhydrous acids and compound ethers to the type of water with the double equivalent, $\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$. We shall then have the following equations:

$$3\left.\begin{matrix}H\\H\end{matrix}\right\}O_2+2PCl_3=3HCl+2PO_3+3HCl$$

$$3\left.\begin{matrix}H\\C_4H_5\end{matrix}\right\}O_2+2PCl_3=3HCl+2PO_3+3C_4H_5Cl$$

$$3\left.\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}O_2+2PCl_3=3C_4H_5Cl+2PO_3+3C_4H_5Cl$$

$$3\left.\begin{matrix}H\\C_4H_3O_2\end{matrix}\right\}O_2 + 2PCl_3 = 3HCl + 2PO_3 + 3C_4H_3O_2.Cl$$

$$3\left.\begin{matrix}C_4H_5\\C_4H_3O_2\end{matrix}\right\}O_2 + 2PCl_3 = 3C_4H_5Cl + 2PO_3 + 3C_4H_3O_2.Cl$$

$$3\left.\begin{matrix}C_4H_3O_2\\C_4H_3O_2\end{matrix}\right\}O_2 + 2PCl_3 = 3C_4H_3O_2.Cl + 2PO_3 + 3C_4H_3O_2.Cl$$

W. G.

II. Miscellaneous Intelligence.

1. *Notices of two minerals from the Lancaster, Pa. Zinc mines;* by W. J. Taylor, (from a letter to J. D. Dana, dated Lancaster, Aug. 31, 1855.)—Allow me to bring to your notice two minerals which I have found in very small quantities at the mines of the Lancaster Zinc Company, a few miles northwest from Lancaster City, Pa.

The first one I will call Tennantite, until I find sufficient of the undecomposed mineral for a thorough quantitative analysis, which will decide the matter conclusively.

Color, steel gray. Streak, dark reddish gray. No crystals have yet been observed; fracture uneven; cleavage not observable in the small pieces found. A qualitative analysis in the wet way showed the presence of sulphur, arsenic, copper, iron and zinc.

The second mineral is Buratite or a variety of Aurichalcite. It occurs associated with calamine and partially decomposed blende, the rock being dolomite, through which there is interspersed occasionally copper pyrites in very small nodules. It is in radiated acicular crystals also laminated, also forming a coating on the limestone (dolomite) and quartz crystals which occur with it. Structure also columnar, which shows when broken the same radiated appearance. Lustre, pearly. Color, verdigris green. B.B. in a matrass gives out water which has neither acid nor alkaline reaction, and the crystals become brownish black. In the inner flame, yellow while hot, and white on cooling. With salt of phosphorus, intumesces and affords a green glass. With equal quantities of soda and borax a globule of copper. Effervesces with HCl, and dissolves entirely.

The ore from the mines of the Lancaster Zinc Co. is principally blende intermixed mechanically in a most peculiar manner with the dolomite rock, in which it is found; at and near the surface it has decomposed into a calamine, it is interesting to observe the process of decomposition very gradually going on.

2. *On Singular Cloud-belts, observed in Georgia, on the 13th of June, 1855;* by Wm. G. Williams, Prof. Nat. Sci., La Grange College, Alabama.—The arches, or belts of cloud observed in June 13th were seven in number, and appeared successively, beginning at about 9 o'clock in the evening. They were seen by me and others at Decatur, Ga. The following facts are from notes taken at the time. The first or northern belt, which may be regarded as the principal one, was first seen at a quarter past nine. It had then the form and appearance of a pillar of cloud, or of dark smoke extending upward from the horizon ten or fifteen degrees north of west, or a little south of where the sun set about two hours previous. At its base it was about two and a half or three degrees wide; its edges were not parallel, but gradually spreading like a

fan partially open. In form it was not unlike the tail of a comet. Its upper extremity was less darkly shaded, and was then at an altitude of about 30 degrees.

Presently we were surprised at its rapid extension in length, not however directly towards the zenith, but obliquely to the horizon along the zodiac, very nearly, if not quite parallel to the ecliptic, leaving the stars Regulus, and Spica Virginis, a little south of it, and finally in the east crossing the milky way considerably south of Altair in the Eagle. In about twenty minutes it reached the meridian, in fifteen or twenty more a point half way down the eastern sky, where it was met by a segment of an arch or belt precisely similar in its form, breadth, color and mode of progress, radiating from a point in the horizon, as near as I could judge, exactly opposite to the base of the western segment, or ten or fifteen degrees south of east, thus forming a complete cloud-like arch or belt, well defined throughout its whole extent; and, although less dense along its middle portions, yet everywhere so dark as to obscure entirely stars of the first magnitude. The darkest portions were about forty-five degrees from the horizon. Its breadth in the centre was about ten degrees, and its southern edge at the moment of its completion, which was about ten o'clock, not far from twenty degrees south of the zenith.

Meanwhile a second belt equally dark, and as distinctly symmetrical as the first, appeared in the west just south of it radiating from the same point, leaving an intervening space of clear sky of some four or five degrees. It was about half as broad as the first; but advanced eastward more rapidly, and in a few minutes made the whole circuit converging to the same point in the east, not however being met by an ascending segment. This was soon followed by a third. Then by another and another till seven were seen at once, each new one farther south, and narrower than the preceding, and leaving narrower spaces. Otherwise, in their general aspect similar, yet not so perfect and unbroken in their outlines. Moreover, although they all terminated in the same points mingling into one in the horizon, yet the last three or four did not seem to shoot forward as from a radiating point, as did the first three. These were rather formed in irregular segments like dotted lines or belts ultimately spreading longitudinally so as to unite.*

It was now half past twelve; but long ere this, to wit, a little past 10 o'clock, or just after the second belt was completed, I noticed that the northern, or original belt was moving laterally to the north, the whole rising upward like the bail of a bucket, on an axis having its poles in the two points of the horizon, where, as before remarked, all the belts terminated. It was followed by the second and third; and it was soon evident that the newly formed and still forming belts went rather to fill up the spaces left by the more northern ones than to advance with them. The rate of motion was apparently uniform, and such as to pass over ninety degrees in somewhat less than three hours.

In their progress, the first belt grew more distinct, and dense; the second less so, but quite variable. During its progress from the zenith to the North star, the former was more regular in its outlines and with

* The apparent radiation of these cloud-belts from the same point was in all probability optical,—that is, simply an apparent convergence in the distance of belts actually parallel or nearly so.—Eds.

its breadth somewhat narrowed in the middle, and its edges thus made much nearer parallel, it was more entire and deeper in its shades of darkness, than at any other period. During this interval the second fluctuated and varied greatly, especially near and just north of the zenith, and seemed to fall behind, spreading laterally, and becoming dim, and in parts, especially near the middle, appearing and disappearing and mingling with parts of the adjacent belts for a time; then again exhibiting distinct and regular outlines. More or less of these fluctuations characterised the other belts to the south during this time.

At half past twelve, or, about the time when the seventh and last belt appeared and arranged itself about 20° above and parallel to the *southern* horizon, then suddenly the original belt ceased moving, and became stationary, resting its lower or northern edge on a line parallel to the horizon, as nearly as I could determine, 20° above it. It now rested just as high and opposite to the seventh—all occupying 140°, or on an average for each belt and intervening space 20°. The first had revolved or moved laterally to give room for the others 90°, and now moved no farther. Its lower edge being still well defined and fixed, I saw a sudden and rapid expansion laterally of the upper edge, growing wider, and wider upward, till, within less than half an hour all the sky, as far as the zenith was filled, all the stars successively disappearing under its deep shades. All the belts now became much confused, and mingled with each other, which seemed to be a signal for their dispersion, an event now evidently taking place.

At one o'clock they were fast disappearing; first near the zenith, and along the middle of the successive belts; then in all directions, dissolving into thin air, leaving their places vacant before the eye. The original belt however, was still lingering nearly entire as late as half past one. Besides this, at that time, only fragments of the other belts remained in the west, and one in the east. There were not then, nor at any time during the whole of this strange phenomenon, were there any clouds visible, other than those forming the belts. On the contrary, the sky, as the belts dissolved, was unusually bright, and the stars everywhere shone with unwonted brilliancy as they also had before the commencement of the phenomenon, and indeed during its progress except in the belts.

Here my observations ended. I learn incidentally that fragments of the belts were seen at Atlanta, till, like clouds, they were tinged with a ruddy glow by the morning sun.

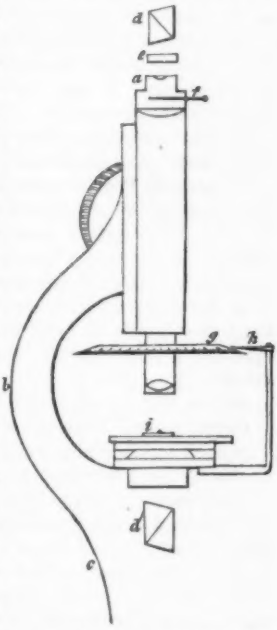
By way of supplement, and in order to throw some light on this subject, I here add the following facts and remarks.

(1.) The phenomenon was followed by a sudden and very great change of weather and atmospheric states as to temperature, moisture, and direction of the wind. For some days previous, it was fair, the air dry and cold for the season, dews generally light, wind north, or northwest. As a proof of these facts I may state that I heard the remark from one or more planters, that the weather was very fine for cutting and saving the wheat; but too cold for the corn to grow. Many were complaining of colds from the low temperature. Others, as myself, put on winter under-clothes. Indeed, on starting to church that evening, I felt the need of, and put on my cloak and wore it till the close of my observations, when I found my thermometer in its usual position

in the house stood at 67° Fahrenheit.* On the next day,† and for successive days, the heat was oppressive, wind south or southwest, air more moist and sultry, dews heavier, and weather unsettled with signs of rain. Beginning with the 17th, there have been here and all around copious showers daily, accompanied with much thunder and lightning, and sometimes driving winds,—that is, for five successive showery days, —the thermometer ranging night and day from 78° to 84°, with remarkable uniformity.

(2.) *It seems to have been local*, so far as yet heard from, and to have been seen farther along an east and west line than one north and south. It was seen in all directions from this place within 10 or 15 miles. It was seen in Cassville, 50 miles northwest. In Campbell Co., some 40 west, and in Newton. Also at Marietta, Ga., by Rev. Geo. White, Dr. Smith, J. E. Shelton, Esq., and many others, whose observation I find on comparison agree substantially with my own.

3. *On a combination of the Stauroscope and Compound Microscope proposed by Prof. von Kobell;* communicated by O. Rood, now of Munich.—In May last, a notice appeared in this Journal giving an account of the stauroscope, an instrument devised by Professor v. Kobell for the recognition of the crystallographical character of minerals and salts by certain optical means. Since then Prof. K. has combined this instrument with the compound microscope so that it is possible to subject microscopic crystals to the same examination as those of a larger size. The arrangement is quite simple and is adapted to any ordinary microscope which is provided with a revolving stage and goniometer circle, it is as follows (see figure‡): a Nicol's prism is attached to the under side of the stage, as in ordinary experiments on polarized light with the microscope; on the stage a glass plate holds the crystal to be examined; just above the eye-piece is placed a crystal of calc spar ground at right angles to the optical axis so as to show the rings and cross, and above this is another Nicol's prism.



The goniometer circle is attached to the *object-glass* by means of a ring fitting to both: to the under plate of the revolving stage which has no rectangular motion, is attached a vernier which will of course move in a circle around the vertical axis of the micro-

* Editor Watchman and Observer of June 21st says:—"Snow fell in Sewell mountain, Va., on the 12th inst."

† Same says in same paper:—"There was a heavy wind and hail storm in Rockingham, Va., on Sunday (17th June) blowing down trees and unroofing barns.

‡ In the figure, *a, b, c*, compound microscope; *d, d'*, Nicol's prisms; *e*, calc spar cut at right angles to the axis; *f*, divided micrometer plate; *g*, graduated circle; *h*, vernier secured to stage so as to turn with it; *i*, glass plate and microscopic crystal.

scope body and consequently of the goniometer circle which is attached to the object-glass; the crystal and vernier move together.

Within the eye-glass in the focus of the eye-lens is stretched a fine thread, or a divided glass micrometer can be used instead; the object of this is the same as that of the lines drawn on the brass plate to which the crystals are attached in the original instrument, viz., to bring a side of the crystal under examination into a certain fixed direction.

The mode of manipulation is the same as with the instrument first described; in this latter form however we have the advantage of seeing at the same moment the position of the crystal and its action on the black cross.

The original experiments were made with a Grunow microscope, which form is peculiarly well adapted for the arrangement described.

4. *Planetary Rotation.*—*Just as this sheet is passing through the press we have received a newspaper slip from Prof.* ORSON PRATT, *Sen.*, dated G. S. L. City, Utah Ter., Aug. 4, 1855, announcing that he has discovered a law respecting the rotation of the planets on their axes, which he thus states: *The cube roots of the densities of the planets are as the square roots of their periods of rotation.*

The author gives no demonstration of the grounds of this principle. In the present state of uncertainty concerning the densities of many of the planets, it may not be practicable to bring the truth of the law to a satisfactory test.

5. *Shark remains from the Coal Formation of Illinois, and Bones and Tracks from the Connecticut River Sandstone;* by Prof. HITCHCOCK, (in a letter to Prof. SILLIMAN dated Amherst, 12th Oct. 1855.)—At the late meeting of the American Association for the Advancement of Science, I exhibited a remarkably fine specimen of a part of the jaw of a shark, allied to the Pristis family, obtained from the Coal Formation in Illinois. After giving the history of the specimen, I submitted it to Prof. Agassiz, who observed that it belonged not only to a new genus, but a new family of fossil fishes, and I have requested him to name and describe it. Its history, locality, &c., I hope to send for your next volume.

Within the present week I have obtained a portion of the bones of a vertebral animal from the Red Sandstone of Springfield, Massachusetts. The rock is the same, and its geological position essentially the same as that in East Windsor, Ct., from which you obtained some years ago similar ones—the only other instance known to me. For these bones I am indebted to the liberality of William Smith, Esq., their discoverer; who has charge of some excavations going on at the United States Armory; and also to General Whitney, Superintendent of the Armory. The larger part of the bones were thrown away by the workmen before they were noticed by Mr. Smith; but I have strong hopes that our anatomists may determine from those that remain, to what class the animal belonged.

Yesterday I procured a slab, weighing nearly a ton, from Roswell Field, Esq., of Gill, containing four gigantic tracks, apparently of a biped, yet accompanied by most distinct traces of a tail. It is certainly one of the most remarkable tracks which I have ever seen, and will probably, (as well as the bones above described,) throw light on the character of the animals that left the footmarks of this valley; I propose

for the name of the animal that made the tracks, that of *Gigadipus caudatus* (γίγας giant and δίπους biped; or biped tailed giant), of this, also, I shall hope to send a description for your next number.

OBITUARY.—Professor J. F. W. JOHNSTON, the distinguished Agricultural Chemist, died at his residence at Durham, England, on Tuesday the 18th of September.

[The following works have been received by us, but on account of the Index for volumes 11 to 20, here added to this number, we are unable at this time to give them extended notices, as well as to insert our usual miscellanies.—EDS.]

6. *Ranking's Half-yearly Abstract of the Medical Sciences.* No 21. Octavo, pp. 300. Philadelphia—Lindsay and Blakiston, June, 1855.—This admirable Semi-annual, contains a very large collection of valuable articles, selected from the current medical literature of both hemispheres, and is particularly full in the Departments of Practical Medicine and Surgery. It is conducted upon much the same plan as Braithwaite's Retrospect, with this exception—its covers furnish *no advertising medium* for *quack medicines.*

It must be an invaluable work for the practical physician whose constant occupation allows him but little leisure to keep himself well informed of the rapid progress which the Medical Sciences are now making.

7. *Elements of Natural Philosophy;* by W. H. C. BARTLETT, LL.D., Prof. of Nat. and Exper. Philosophy, U. S. Military Academy, West Point, author of "Elements of Mechanics," "Acoustics," "Optics" and "Analytic Mechanics:" IV. Spherical Astronomy, 466 pp. 8vo. New York, 1855. A. S. Barnes & Co.—The high reputation of Prof. Bartlett is a sufficient guaranty for the excellence of his works.

8. *Mathematical Dictionary and Cyclopedia of Mathematical Science,* comprising definitions of all the terms employed in Mathematics, an analysis of each branch, and of the whole as forming a single Science, by CHARLES DAVIES, LL.D., author of a complete course of Mathematics, and WILLIAM G. PECK, A.M., Assistant Professor of Mathematics, U. S. Military Academy, 592 pp. 8vo. New York, 1855; A. S. Barnes & Co.—This work has been prepared with care and is well illustrated with figures.

9. *Annual Report of the Superintendent of the U. S. Coast Survey for* 1854. pp. 4to, with a large number of maps and plates. This volume besides its beautiful maps, and its coast information, contains various memoirs of great value on Tides, the Gulf Stream and other subjects, by Prof. A. D. BACHE, Superintendent, and additional papers by different persons connected with the survey. Some of these chapters we may cite in a future volume of this Journal.

10. *Report on the Minerals and Mineral waters of Chile;* by J. LAWRENCE SMITH, Prof. Chem. Med. Depart. Univ. of Louisville. 25 pp. 4to.

11. *Esquisse Géologique du Canada, pour servir à l'intelligence de la carte géologique et de la collection des Minéraux économiques, envoyées à l'Exposition Universelle de Paris,* 1855; by W. E. LOGAN, Member of the Royal Society of London, etc., and T. STERRY HUNT, Member of the Geological Society of France, &c. 100 pp. 12mo. 1855. Paris: H. Bossange et fils.

Conrad's Eocene fossils. No 4

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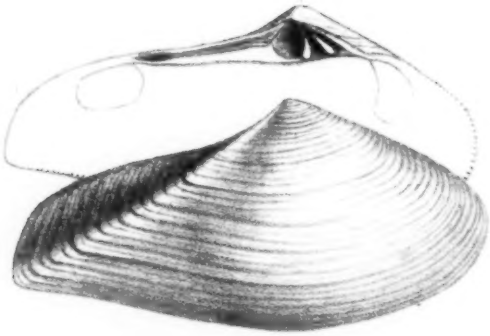

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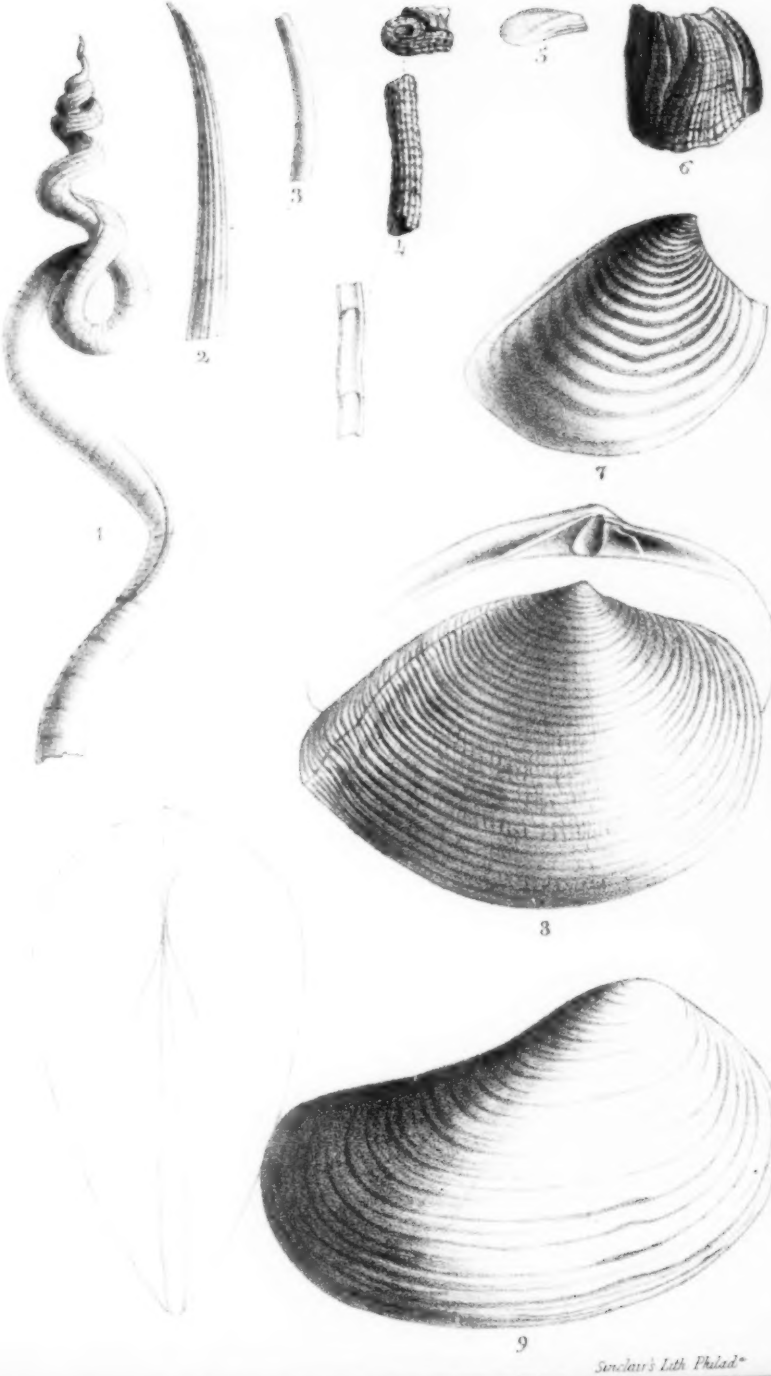

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Conrad's Eocene fossils. Pl. 3.

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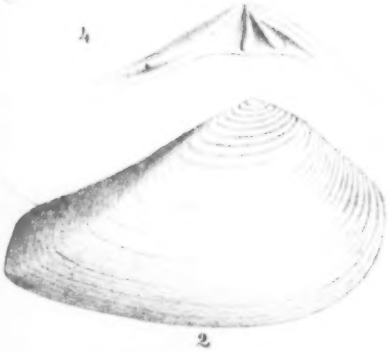

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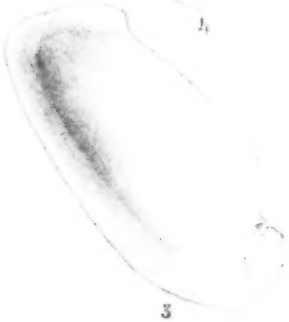

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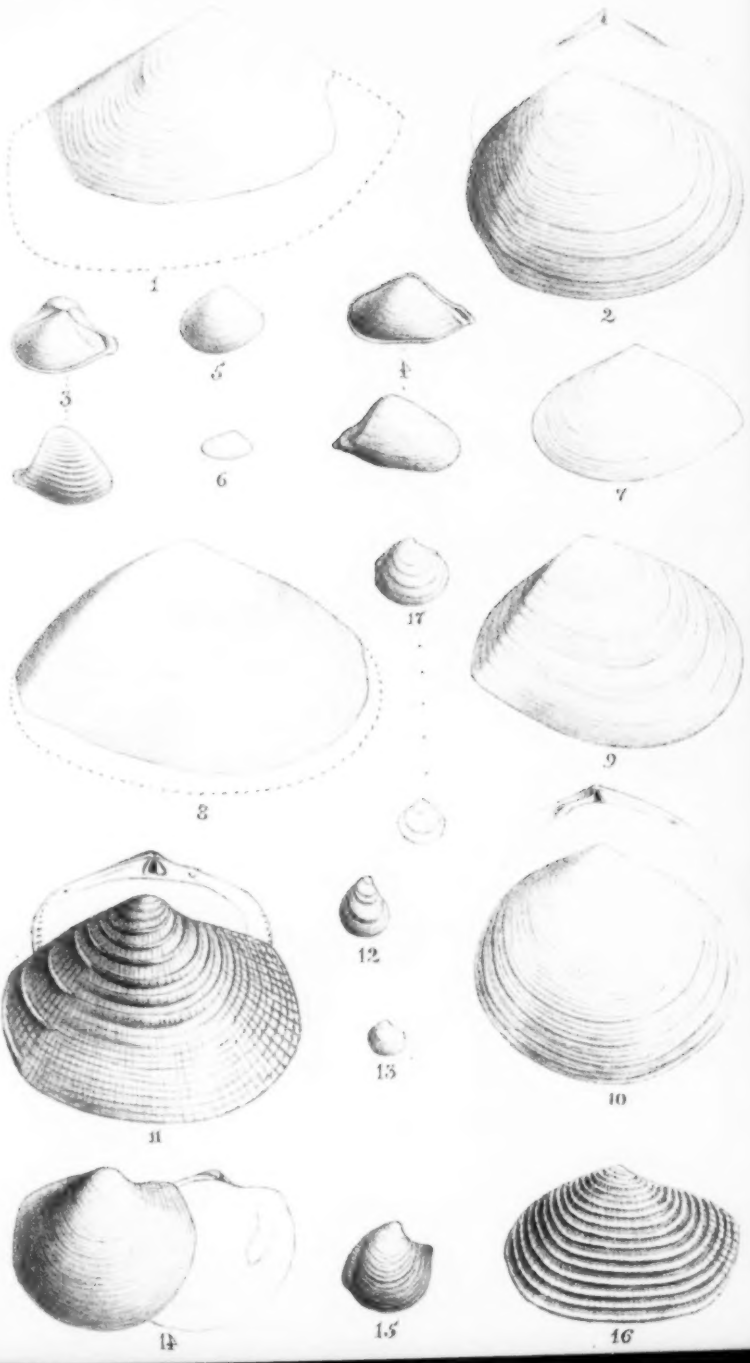
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